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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Contributions to Practical Surgery.* By WILLIAM STOKES, jun., Surgeon to the Meath Hospital and County Dublin Infirmary; Lecturer on Surgery, Carmichael School of Medicine.

On the Resection of Joints and Bones.

1.—RESECTION OF THE ANKLE-JOINT.
2.—SUBPERIOSTEAL RESECTION OF THE ELBOW-JOINT.
3.—RESECTION OF THE KNEE-JOINT.
4.—RESECTION OF METATARSAL BONES.

THERE are few procedures in operative surgery which have so many warm supporters or such bitter adversaries as resections of the larger joints. This difference of opinion among surgeons in regard to the value of these operations is due to many circumstances, among which may be mentioned the frequent occurrence of unsatisfactory results, and the unlimited faith which so many surgeons have in the value of rest in the treatment of all forms of articular disease. No one could be more convinced of the value of rest than I am; but I cannot but think that the surgeon who relies solely on this in the treatment of many forms of articular disease, rejecting all idea of operative interference until the vital powers of the patient

become so seriously impaired as to render success in operating highly problematical, fails in duty to his patient, and to the cause of true conservative surgery. It is, doubtless, this tendency among surgeons to make operative interference the *dernier ressort* that has been so often the cause of want of success in these procedures, and brought them consequently into an ill-deserved disrepute. To the resection of the ankle-joint a special class of objections have been urged, based, first, on the mechanical difficulty of the operation; and secondly, on the difficulty, and, according to some, on the impossibility of determining, even approximately, the amount of disease that may have to be removed. With regard to the first of these, it is unnecessary to dwell, for no mechanical difficulty should ever deter the surgeon from performing an operation that may be of ultimate advantage to the patient; and with regard to the second, although it cannot be denied the cases are very rare in which carious disease of the ankle-joint is sufficiently localized to justify resection, yet, should the disease be found too extensive to indicate this procedure, there is then nothing to deter the surgeon from amputating at the ankle-joint after the manner recommended by Professor Syme or Pirogoff.

Owing to the recent researches, practical as well as theoretical, of Professors Langenbeck and Hancock on the great surgical importance of resection of the ankle-joint in cases as well of injury as of disease of that articulation, the subject naturally is regarded by the surgical profession as one worthy of deep care and consideration. I have little doubt, therefore, that the particulars of the case in which the operation was performed by me, for the first time in Ireland, and which I am about briefly to record, will, in no small degree, be of interest to my professional brethren. Before, however, discussing this case, it may not be considered unimportant or devoid of interest to glance briefly at some of the particulars of the earlier cases in which this operation was performed.

In a paper by Professor Langenbeck, which he kindly sent me, on some cases in which he performed resection of the ankle-joint for gun-shot injuries received during the Schleswig-Holstein campaign of 1864, and also by the late Professor Velpeau [Med. Operat. 2 T. p. 739], it is stated that the first case in which resection of the ankle-joint was performed was at the battle of Fontenoy, in 1745, by a surgeon named Réad. The case, however, although of extreme surgical interest, should, I think, hardly be recorded as an example of the resection of the ankle-joint in the ordinary acceptation of the

term. The following are some of the leading particulars of this remarkable case:—"Charles Amiens, a soldier belonging to the company of Gurerduc in the Beauvoisis Regiment, was struck, in the battle of Fontenoy, by a cannon ball, which carried off the lower extremity of the fibula. The inferior extremity of the tibia was fractured in several pieces, the injury also involving the astragalus, the ligaments, and other structures of the ankle-joint. The foot was completely displaced backwards and inwards. M. Réad's first idea was to amputate immediately, but owing to the entreaties of the patient not to do so, and there being no immediate danger, the operation was deferred. Several fragments of the bone were then extracted of the inferior extremities of both fibula and tibia, as well as some portions of the astragalus. The wound was then dressed, the foot being supported by a suitable apparatus, which was not removed for four days. No accident occurred during the patient's convalescence, which lasted about four months. When he left the hospital of Valenciennes to go to the Invalides, he was able to walk without any support. There was no re-formation of the portion of the fibula which had been removed."

The next case occurred in the practice of the elder Moreau, in 1792. The case was one of extensive caries of the left ankle, which was induced by a severe sprain the patient received about a year before coming under the care of M. Moreau. "There was a fistulous ulcer on each side of the joint, from which a sanious and fetid pus was discharged; and the probe being introduced through the openings, the articulating surface of the tibia, as well as that of the fibula, and the body of the astragalus, were felt to be bare. The foot and lower part of the leg were swelled. There was a dull pain in the diseased part, and the patient could not lean his weight on the limb."[a] As there is apparently great confusion and probable inaccuracy in the description of the operation, we shall not enter into any discussion as regards the procedure. The result, however, of this operation we may state; the patient was not "able to lean his weight upon the foot till the sixth month after the operation. During the seventh he used crutches. In the eighth month he could walk with a stick, and by the end of the ninth month he walked without any assistance whatever, and in such a way that he could do what he pleased!" It is to be regretted that although the result of the case is described as being "surprising," no mention is made as to

[a] Cases of Excision of Carious Joints. By P. J. Moreau. Trans. by J. Jeffrey, M.D., Glasgow, 1806.

whether any sinus or other evidence of existing disease remained after the operation. As a detailed account of the cases of greatest interest in which the operation of excision of the ankle-joint has been performed since the time of the Moreaus, has been given by Professor Hancock in his deeply interesting and valuable Lectures on the Surgery of the Human Foot, delivered at the Royal College of Surgeons of England in last June, it is unnecessary for me, on the present occasion, to give a renewed account of them. As far as I can determine, it appears that the operation has been performed fifty-one times, and of these thirty-eight have been successful, and thirteen unsuccessful, which gives about seventy-five per cent. of successful cases. This will be seen by a glance at the accompanying table, a great number of the cases in which I am indebted to Professor Hancock for kindly furnishing me with. I may mention in passing that the cases in this table include all those in which the operation was performed as well for injury as disease:—

EXCISION OF THE ANKLE-JOINT.

Name of Surgeon	Cases	Successful	Unsuccessful
Réad, . . .	1	1	0
The Moreaus, .	2	2	0
Jœger, . . .	1	1	0
Gooch, . . .	1	1	0
Textor, . . .	1	1	0
Buchanan, . .	1	0	1
Hancock, . .	5	4	1
Jones, . . .	1	1	0
Statham, . .	2	2	0
Catton, . . .	1	1	0
J. Holmes, . .	3	2	1
Langenbeck, . .	9	8	1
Humphrey, . .	2	2	0
Simon, . . .	1	0	1
Erichsen, . .	5	3	2
Hussey, . . .	1	0	1
Morton, . . .	1	1	0
Canniff, . . .	1	1	0
Burnell, . . .	1	0	1
Paget, . . .	3	2	1
Canton, . . .	1	0	1
C. Heath, . .	1	0	1
Moore, . . .	1	1	0
Davis, . . .	1	0	1
Delagarde, . .	1	1	0
Neudörfer, . .	1	1	0
Sédillot, . . .	1	1	0
Bœckel, . . .	1	1	0
	51	38	13

We may now proceed to discuss the particulars of the case in which I performed the operation of resection of the ankle-joint last April. H. M'G., aged twenty-seven, was admitted into the Meath Hospital, under my care, on the 10th of last April. He stated that about eight years previously he received a kick of a horse over the inner malleolus. The joint swelled after this, and was extremely painful. This seems to have subsided under suitable antiphlogistic measures. The swelling, however, never entirely disappeared. After some time abscesses and sinuses formed, which never healed, and from which purulent matter, varying as well in consistence as in quantity, continually oozed up to the time of his admission into hospital. In other respects the patient was in good health, though, as might be expected, much debilitated by the long confinement and the continuous discharge from the sinuses. On probing these, I found that the direction of all was towards the astragalus, a circumstance which went far in making me come to the conclusion that the disease was probably limited in extent, and possibly localized in the astragalus and articulating surface of the tibia. Having formed this opinion, I deemed the case a fitting one for excision, and I proposed it to the patient, who gladly acceded, and, my colleagues concurring in the propriety of the operation, I performed it on the morning of the 29th of April, in the following manner:—

A long vertical incision was made along the posterior edge of the fibula, and then brought obliquely forwards and inwards towards the internal cuneiform bone. A somewhat triangular-shaped flap was then dissected backwards, a step in the operation which was attended with much difficulty, owing to great matting and thickening of the soft parts. This having been done, the next step was to excise the end of the fibula. This was effected with great difficulty, owing to the fibula being firmly anchylosed below with the tibia. The outer portion of the extremity of the fibula was found softened and diseased. A chain saw was then passed round the fibula about two inches and a-half above its extremity, and the bone divided. Owing, however, to the firm anchylosis below, it could not be removed until a section was made, separating it from the tibia, by one of Langenbeck's small resection saws. This having been done, the excised portion of the fibula, with a small portion of the outer edge of the tibia, to which it was attached, was easily removed. It then was obvious that not only the extremity of the tibia, but also the astragalus, was extensively diseased. The greater portion of the latter was removed, chiefly by the straight and rectangular gouges. As

we saw then that the inner malleolus was extensively engaged, we deemed it advisable to attempt the dislocation of the tibia outwards through the wound. Mr Porter, whose assistance at this and every stage of the operation was invaluable, made the attempt to dislocate the extremity of the tibia through the wound. It was found impossible. More of the diseased structures were then removed, sometimes by the gouges, Langenbeck's and Hey's resection saws, and also by an ingeniously-constructed American forceps, of great power, which was kindly lent to me by my friend Dr. E. O'Grady. A second, and then a third attempt was made to bring the extremity of the tibia through the wound. The last attempt proved successful, and our efforts were well rewarded, for it was plain that had we trusted to the instruments we had previously been using, we never could have removed all the disease we found situated in and about the inner malleolus. The accompanying chromolithograph, executed by Mr. Lewis with his accustomed skill, from a drawing by Mr. Burnside, represents accurately the condition of the diseased osseous structures which I removed. There was no necessity found for removing the periosteum by any of the raspatoria for the purpose, for it was found detached and thickened, lying loosely on the surface of the bone. A slice of the tibia, about a quarter of an inch in thickness, was then removed by an ordinary amputation saw, and the resection was then complete. One vessel only had to be secured, which I did by acupressure, adopting the method of "half rotation," or first variety of the fifth or Aberdeen method. Nothing could have been more entirely satisfactory than the way the hemorrhage was arrested by this procedure. A piece of dry lint was then placed in the wound, and the edges brought together by five or six points of silver wire suture. A gypsum bandage was then applied from the toes to the junction of the upper with the middle third of the leg, in the manner I have already described in the *Dublin Quarterly Journal* for May, 1865, the particulars of which I need not therefore now enter into. The patient was then placed in bed, with the limb slightly elevated, and ordered forty drops of Battley's sedative, in a little sherry.

8 P.M.—Pulse 136; suffered from considerable abdominal pain during the day; no sleep as yet; no pain in ankle. Ordered a draught with sol. mur. morphiæ, gtt. xxxv., in an ounce of cinnamon water.

April 30, second day, 10 A.M.—Had a restless night; pulse 140; had several attacks of retching during the night; ordered iced beef-

tea and champagne to be taken in small quantities every second hour; also, the following draught to be taken at once:—

℞ Liq. mur. morphiæ, gtt. xxxv.
Spt. chloriformi.
Tinct. cort. aurant, aa ʒss.
Aquæ lauro cerasi, gtt. xx.
Aquæ, ʒvii.
Fiat haustus. ss.

4 P.M.—Found the patient much quieter, but pulse is still 140. I cut a large oval-shaped window in the gypsum bandage over the situation of the wound, and then applied, with a large camel-hair brush over the bandage, a solution of Dammar resin in sulphuric ether, to prevent the action of the water in the continual bath from softening the gypsum. The limb was then placed in the bath, being supported in it by a coarse net. This gave the patient great relief; the pulse fell almost instantaneously to 120.

May 1, 9 A.M.—Had a quiet night; pulse 120, full and strong; no pain in ankle; had several hours sleep.

3.—Pulse 100; skin cool; tongue moist. Free suppuration from the wound; appetite good.

On May 14, I took the limb out of the bath, removed the gypsum bandage, and placed the limb in a box splint. The wound was then dressed with Dr. Richardson's styptic colloid, which I continued to use for upwards of a fortnight. I have had much reason to be satisfied with it, not only in this but in some other cases in which I have employed it. On May 29, the wound being nearly healed, I took the limb out of the box.

I shall not, on the present occasion, discuss the various modes which have been suggested and practised by surgeons for performing the operation of excision of the ankle-joint. The method which I adopted differs from those recommended originally by Moreau, and subsequently by Professors Hancock, Langenbeck, and others. The operation, as performed by me, leaves the extensor tendons intact. There is no risk either of injuring the tendon of the tibialis posticus muscle, or posterior tibial artery, and as there is only a single incision required, the chances of rapidity of healing are increased, as the suppurating surfaces are necessarily less in extent. Lastly, there is no danger of wounding the anterior tibial artery.

Towards the end of August the condition of the joint was as follows:—The wound was completely healed, but along its track were two or three openings of sinuses, through which oozed

occasionally a little thin purulent matter. In every other respect his health was excellent. He was quite free from all pain, and was beginning to be able to walk a little with the aid of sticks. In October the sinuses were completely healed, and the patient was able to walk up and down the ward without any inconvenience. The immediate results, therefore, of the operation were completely successful, and I was most sanguine that I would be able to add this to the list of cases said to have been permanently cured by the operation. I have recently, however, heard from the patient in the country that the sinuses which were so long closing have re-opened, which, in all probability, is evidence of a return of the carious process in some one or more of the bones of the foot. It is, I think, much to be regretted that the eminent surgeons who have published so many "successful" cases of resection of the ankle-joint for caries, have not stated at what period after the operation they were recorded as such. It would, of course, be unphilosophical to generalize on the value of any surgical procedure from the result obtained in a single case; but I cannot help being inclined to the opinion, that although I believe the operation is specially indicated in gunshot and many other grave injuries of the ankle-joint, the cases in which the operation is suitable for the removal of carious disease must be very few indeed, in consequence of the great rarity of meeting with cases in which the disease is sufficiently localized to justify this procedure.

II. *Sub-periosteal Resection of the Elbow-joint.*—There can be little doubt that of the articulations, for disease of which, resections are now so frequently practised, the elbow-joint must be held to be the one which is specially suited for this operation, inasmuch as unfavourable results are of such rarity, and also from the fact that the operating surgeon is now in a position to expect, with good reason, not only the subsequent attainment of perfect flexion and extension in the joint, but also a re-formation, more or less complete, of the removed osseous structures which, in the great majority of cases, undoubtedly can be attained, either by careful preservation of the periosteal fibrous envelope, or, when it is possible, by the method of *évidement*, which has been recommended in preference by the eminent Strasbourg surgeon, M. Sédillot. A glance at the table of 333 cases of excision of the elbow-joint which have been collected from various sources, and published in the "Archives of Clinical Surgery," Vol. vi., p. 86, by Dr. Dontrelepont, will at once

satisfy the most sceptical surgeon in regard to the great rarity of unfavourable results in these cases. They are as follows:—

In 333 cases—

Death,	40, or	12 per cent.
Amputation,	14, „	4·2 „
Anchylosis,	23, „	6·9 „
Mobility,	256, „	76·87 „

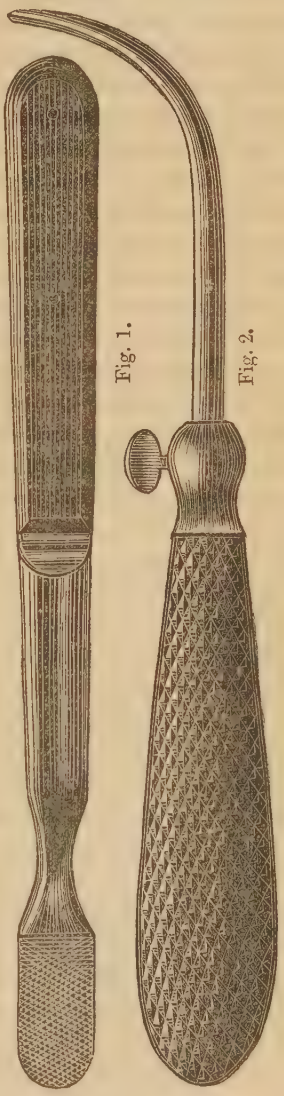
In truth, the operation very seldom indeed terminates fatally, and when it does so, it is from pyemia or other causes, which are common to all operative procedures. The only instances which have fallen under my observation, and which terminated fatally, were two. One in which there was acute inflammatory disease in the joint at the time of the operation, and the other was one in which the operation was performed—I may add, not in this country, to our credit be it said—for no disease, but for a congenital malformation of the elbow, which impeded, to a certain extent, the natural motions of the joint. In this latter case the patient died of pyemia on the eighth day after the operation.

The particulars also of the case of C. Waddick, whose left elbow-joint I excised, and the account of which I published in the *Dublin Quarterly Journal* for May, 1865, fully bear out the truth of the statement made above in regard to not only the possibility but probability of obtaining subsequently perfect mobility in the new joint. The patient now enjoys every motion in the elbow that the healthy joint possesses, perfect power of flexion, extension, pronation, and supination. The osseous reformation also is very remarkable. Encouraged by the success attained in this case, and which I have seen in many cases equally well attained by other surgeons, I determined to give the patient, whose case I am about briefly to record, the chance of obtaining, by similar means, the possession of a useful arm, and, as far as the case has gone, though in many respects an unfavourable one, owing to the large amount of disease which was found external to the joint, it promises to fulfil these not too sanguine expectations.

Catherine Ryan, aged eight, a child of very delicate strumous temperament, was admitted under my care into the Meath Hospital, on the 24th of last June. The right elbow, which was evidently the seat of very extensive scrofulous disease, had been affected for upwards of nine months, and had commenced, according to the accounts her parents gave, apparently without any local exciting cause. The joint was enormously enlarged, the result apparently

not so much of any extensive effusion as of great thickening and hypertrophy of the synovial and other soft structures. Though, in truth, in all such cases, when the disease is extensive, it is difficult, if not impossible, to define accurately what parts are chiefly affected. The integuments, especially on the posterior aspect of the joint, were much disorganized, and in three or four places had ulcerated, and, through the sinuous openings in these situations, oozed continually a thin, sanious, unhealthy-looking, purulent discharge. Through only one of these sinuses was I able to satisfy myself of the existence of diseased bone, namely, through the one situated over the head of the ulna. The patient did not suffer much pain in the joint except when it was moved. All power of flexion and extension was completely and entirely lost, and, in truth, at first sight, it was quite obvious that in the treatment of the case, looking at the large amount of disease that was present, the question to determine should be whether it was one for which the operation of amputation or excision should be performed. The latter I determined on, as being beyond question the preferable alternative, having stated, however, previous to the operation, that should I find the amount of disease too great to justify resection, I should then have recourse to amputation of the arm. The operation, which was performed on the 25th of last June, was done in the manner I have frequently advocated, namely, by the vertical incision of Park, taking especial care in detaching the soft parts from the bones, to remove along with them, by some of the various ingeniously constructed instruments for the purpose, the fibrous envelope or periosteum. The advantages which I have pointed out as accruing from the adoption of the vertical incision are, first, that the suppurating surfaces are necessarily less in extent than when the ordinary H incision is made, and that consequently the chances of rapidity of healing will be increased if the opportunity for suppuration be diminished. Secondly, that the numerous muscles in the neighbourhood of the elbow joint are not divided transversely, and that, consequently, there can be no transverse cicatrix in their tissues, which would, in all probability, impede their action subsequently; and lastly, we avoid, by the vertical incision, the formation, of a transverse cicatrix, which, when it exists, impedes certainly the extension and flexion motions of the joint. In preserving the periosteum we have a two-fold purpose. First, to obtain a more natural appearance in the shape of the joint; and, secondly, which is far more important, to obtain a genuine

re-formation of bone. The periosteal ablation I accomplished in this case by a periosteal raspatorium, devised, I believe, by Professor Langenbeck, and which was kindly given to me by Dr. O'Grady. As I have found it a most useful instrument in this and other cases where I have had to detach this membrane, I affix here (Fig. 1) a wood-cut of Langenbeck's periosteal raspatorium, and also of one (Fig. 2) devised by M. Ollier of Lyons, in order to give my readers a clear idea of their shape and construction.



And here I would certainly join issue with MM. Sédillot, Sarazin, Marmy, and other of M. Sédillot's followers, who have asserted with more energy than reason the vast superiority of the method of *évidement* or "scooping" over sub-periosteal resection, practised in the manner I have described. M. Sédillot's theory is based on the belief that for the successful regeneration of bone the preservation of the fibrous envelope of the bone is of less importance than its external dense compact tissue. It is impossible, according to him, to remove the membrane from the diseased bone without seriously injuring or destroying its osteo-genetic properties. He believes that the success of osseous regenerations depends on "two principal causes. First, the integrity of the periosteum; second, the regularity and immobility of the surfaces, sheaths, or moulds, in which the osseous matter is formed.

"Thus is explained the rapidity or slowness of the generation of bone, by the diverse degrees of alteration or destruction of the periosteum (by wound, inflammation, ulceration, suppuration, gangrene) and the immobility and regularity of the surfaces where

the osseous cellules are deposited and agglomerated, serve to explain the superiority of the method of scooping (évidement) over that of subperiosteal resection, because in the former the mould is regular, immovable, and invariable, and the periosteum intact, while in the latter, this membrane is always more altered, sometimes destroyed, and the mould incomplete, mobile, and irregular."[a]

Without entering minutely into the various objections, theoretical as well as practical, that might with good reason be urged against this theory, I may observe that the "évidement" to be performed in the manner indicated by M. Sédillot, presupposes, of necessity, a case in which the disease is localized, to a greater or less extent, in the central cancellated tissue of the bone; and, though it cannot be denied that this tissue is frequently, though not always, the primary seat of strumous disease, I would ask, is it not of extreme rarity that the surgeon has an opportunity of examining, after operation, a case of strumous disease of any of the joints in which it was localized in this situation? and were the surgeon enabled to diagnose such a case with accuracy, would he then be justified in recommending resection, when the disease was necessarily so limited in extent? In regard to the statement made by M. Sédillot, concerning the injury that is inflicted on the periosteum, which, in his opinion, has the effect of impairing, if not of destroying, its osteo-genetic properties, the best answer that can be given is, that the facts of a large number of recorded cases, as well as the results of extensive physiological experiments, are entirely at variance with this purely theoretical objection.

To return, however, to the case which has suggested the above remarks. On opening into the joint I was at once struck with the limited extent of disease in the osseous tissues. It was, in fact, limited to the head of the ulna, but in this situation the cartilages were pulpy and thickened, and the bone soft and degenerated. Although the disease was so limited in the osseous structures of the joints, I had no hesitation in removing the ends of all the bones entering into its formation, as the results of partial excisions have been found to be so unsatisfactory. For example, the difficulty of getting proper union is very great in such cases, and when it is obtained it is usually by bony anchylosis instead of by fibrous tissue. The excision being performed, the edges of the wound were brought together by five or six points of silver suture, and a

[a] Gas. Hebdomadaire, January 27, 1865. Translated in Syd. Soc. Surgical Report for 1865-66.

piece of oiled lint being introduced into the wound to serve as a vent for the purulent discharge, the limb was put in a semi-flexed position in a gypsum bandage. The arm was first enveloped with a flannel roller from the wrist to the shoulder, a large padding of French cotton having been placed over the wound, it was next covered with a coarse muslin bandage, into which I had previously rubbed some dry plaster of Paris powder. This bandage before being applied over the flannel one was moistened with tepid water. A layer of semi-fluid plaster was then laid over the muslin bandage, and this, when nearly dry, was covered with a second muslin roller, and the bandage was completed by laying on a second application of gypsum.

The following day I cut an oval window in the bandage over the wound, and having painted over the bandage with an etherized solution of Dammar resin to prevent the action of the water softening the gypsum, I placed the limb in the continual bath. In this the arm was kept for twenty-two days. It was then taken out and put up in a felt splint, and the ulcerations, which existed previous to the operation, I commenced dressing with a solution of iodine and glycerine. After some weeks of this treatment the ulceration healed. I shall not detail the daily progress of this case. In the early part of last October the patient, though her present health was in a very delicate state, left the hospital, the wound and sinuses having completely healed.

III. *Resection of the Knee-joint.*—Notwithstanding the energetic and able advocacy for resection of the knee-joint in the Dublin school, the operation is one which all reflecting surgeons must admit to be still *sub judice*. We cannot shut our eyes to the fact that in many of the leading Continental schools of surgery the operation is in disfavour. In Berlin, for example, we see it entirely abandoned by the deservedly eminent Prussian surgeon, Professor Langenbeck, and in the Vienna and Paris schools it is also held in disrepute. From Dr. Eben. Watson, formerly surgeon to the Royal Infirmary of Glasgow, we learn that of the eleven operations for excision of the knee-joint that have been performed there up to 1860, only four were successful, of the seven failures, "four died from the effects of the operation, three were submitted to amputation through the thigh, and one of them survived, so that of the eleven patients operated upon six died.[a] In a recent

[a] Glasgow Medical Journal, October, 1859.

number also of the *Medical Press and Circular* we were informed by Mr. Holmes Coote, that of the six cases of excision of the knee-joint, which were performed at St. Bartholomew's Hospital last year, three died, and in all cases it was performed at an age most favourable for convalescence and recovery—namely, between infancy and early adult life." In gunshot wounds involving the knee-joint the results of excision have been still more unfortunate. Of the seven cases recorded prior to the recent American war, only two terminated successfully, and of the eleven cases in which complete excision of the knee-joint was performed during the late protracted war, as recorded in Circular No. 6, a report of the surgical experiences acquired there, and recently published by the American Government, only two were successful, and in one of these the author of the report, Surgeon-General Dr. Barnes, states that the success was "so extraordinary as to suggest some doubts as to its authenticity." We should not forget, however how very difficult it must be during active military service to obtain the requisite hygienic and surgical resources necessary for the after treatment of such cases.

Notwithstanding these unfavourable statistics, the operating surgeon can always derive encouragement from the records of the cases, collected from various sources by Mr. Butcher, for he will find that of the ninety-nine cases in these tables only twenty-five deaths are recorded, and of the remainder, twenty-four were "under treatment," and said to be recovering, and forty-two were "cured." In truth, one chief cause for the ill-success which now so often attends this operation is, I think, that surgeons have too often looked upon the procedure as an alternative for amputation, and very rarely, indeed, as one of expediency. In other words, it is too often deferred until even the propriety of amputation might properly be questioned.

The particulars connected with this case I am about very briefly to record, have suggested these preliminary remarks; for I cannot but think it more than probable, that had the operation been performed at an earlier period, when the vital powers of the patient were not brought to so low an ebb, the chances of bringing the case to a successful termination would have been largely increased.

Susan Hillas, aged twenty-three, was admitted into the Meath Hospital in last February, having been recommended to me by my friend and colleague in the Carmichael School, Dr. R. Shaw. Her condition on admission into hospital was truly pitiable. There was

great emaciation, an expression of long-continued suffering, and much prostration from a continuance of sleepless nights, produced by the often repeated and violent starting pains, so characteristic of the disease, which were present both by day, on making the slightest movement, and also by night, when they occurred with a violence and frequency which caused her often to scream, so painfully acute were they. The knee, which had been affected in this way for upwards of eighteen months, was not much swollen, being free from effusion, but over the inner side of the joint there was the peculiar elastic, indian-rubber sort of sensation, which is so often present in articulations affected with much synovial thickening. The joint was strongly flexed, and any attempts to alter its position were wholly unavailing, owing to the extreme pain that they produced. There was no physical sign or symptom of any organic disease elsewhere to be found. Shortly after the patient's admission into hospital, I put her under the influence of chloroform, and extended the leg, and kept it in that position by an apparatus, which I made for the purpose, of Spark's prepared leather. In addition to this, I kept continually applied to the joint the belladonna liniment of the Pharmacopœia, and internally administered opium and cicuta in large doses, during both day and night.

Under this treatment the patient appeared for some time to derive benefit, but it was not permanent, and after trying rest and the other treatment I have already alluded to, for more than two months, and not finding any improvement in the patient's general condition, I determined, should she accede, to have recourse to some operative interference. The patient promptly and positively refused to have an amputation performed, although I expressed my opinion that, owing to her very debilitated condition, I thought the chances of subsequent convalescence would be greater if she selected amputation, in preference to excision, of the knee-joint. To the latter procedure, however, she would only accede. Nor, I confess, was I at all despondent about bringing the case to a successful termination, for there were some features connected with it which, I thought, made the resection of the knee-joint a perfectly fitting and justifiable operation to have recourse to. Among others, her freedom from disease elsewhere, and above all, her youth. Resection, therefore, being agreed upon, the next important question for me to determine was, what line of incision I should adopt. As I was very anxious to preserve the patella, should that bone and its

cartilage be found healthy, I anxiously considered the propriety of performing the operation by the single longitudinal incision, one great advantage of which is, that the ligament of the patella and its attachment to the tibia can be preserved intact. This line of incision was formerly strongly advocated by Langenbeck and his pupils, and having frequently practised it on the dead subject, I can endorse all that has been said as regards the facility with which the operation can be performed by adopting this method.

It is performed in the following manner:—The limb should be extended, and an incision, commencing a little above the internal condyle, should be brought down on a line with the anterior edge of the internal lateral ligament, as far as the insertion of the sartorius. The knife should be carried down to the bone, the capsule of the joint divided on the inner side of the femur, and the patella then dislocated completely outwards. In this way a complete view of the internal structures of the joint is obtained.

The crucial ligaments should then be divided, and the joint forcibly flexed, and the external lateral ligament divided. The remaining steps of the operation can be performed in the ordinary manner.

This method, however, has not been attended with as favourable results as the old H-shaped incision, and accordingly I determined to adopt this. It would have been quite useless (as I found on opening the joint) to attempt preserving the patella, for the bone, as well as the cartilage on its posterior surface, was extensively diseased.

During the operation, which, ably assisted by my colleagues Messrs. Porter, Collis, and Wharton, I performed on the first of last May, three vessels, two of considerable size, had to be secured, which I accomplished by acupressure, adopting the methods of quarter and half rotation, or first and second varieties of the fifth or Aberdeen method. The hemorrhage was perfectly arrested by this. There was considerably more disease in the joint than I had anticipated. The synovial structures were pulpy, thickened and gelatinous, and there was extensive ulceration of cartilage. The case was, in truth, a typical example of ulcerative action in articular structures. This was especially well-marked over the inner condyle, the cartilage of which was much eroded, as was also the bone. The same condition was observed on the corresponding facet on the head of the tibia; and, as was before mentioned, the cartilage on the posterior aspect of the patella was extensively engaged. There was intense

vascularity of all the structures of the joint. In the accompanying chromolithograph, from a drawing by Mr. Burnside, their appearances are faithfully delineated.

After the operation was concluded, the limb was put up in the ordinary manner in Butcher's box, and a powerful anodyne administered at once.

3 P.M.—Found patient in a state of great excitement and hysteria. Heart's action fluttering, and very excited. Impossible to estimate accurately the rate of pulse. Complains of great starting in limb. Ordered an anodyne, with forty drops of Battley's sedative.

10.30 P.M.—Quieter; excitement abated; has had a little sleep; feels drowsy; does not complain of startings or pain.

May 2nd, 10 A.M.—Pulse 140; great gastric irritability; vomits everything; ordered iced soda-water, hydrocyanic acid and champagne.

9 P.M.—Has had another very severe fit of vomiting; nothing remains on stomach; pulse 150, weak and compressible; appears to be sinking; ordered a draught containing sol. mur. morphiæ, gtt. xxxv.; acid hydrocyanici, dil. gtt. vi.; brandy, ʒi., in two tablespoonfuls of iced soda-water; and also a small blister to the pit of stomach.

3rd.—Great change for the better; had some hours sleep; countenance cheerful; retching and vomiting ceased; pulse 112; to continue the iced beef-tea and champagne in small quantities at a time.

4th.—Dressed the wound for the first time; edge of anterior portion of flap united; all going on well.

6th.—No return of gastric irritability; pulse 120; tongue moist; appetite good; copious suppuration from the wound; removed two of the points of suture; transverse wound united completely.

It would be tedious to continue the daily report further. For upwards of a fortnight after the last report, notwithstanding the extremely hysterical and irritable temperament of the patient, everything appeared to promise well. The suppuration in the wound continued to diminish sensibly; the appetite also improved, and the patient had better nights; at this time I was most sanguine in believing that the case would be brought to a successful termination; my colleagues were also of this opinion. At this stage, however, grave pulmonary symptoms manifested themselves—the pulse rose, and night perspirations, and other evidence of a hectical condition were developed.

On the 6th week the pulmonary symptoms, which exhibited all the usual phenomena of acute tuberculosis, became much aggra-

vated, and it became evident that the end of the patient was rapidly approaching. Notwithstanding, however, her critical condition, the wound continued to improve up to the last week of her life; but on this week, the seventh after the operation, she gradually sank, and died on the 15th of June.

In considering the features of this case, I cannot avoid coming to the conclusion that had the operation been performed at an earlier period of the disease, before the patient's vital powers became undermined by long-continued suffering, the result would have been probably very different, and I regret—and think it right to record that regret—that I did not perform the operation when the patient first came under my care at the end of last February. We should not wait until resection becomes nothing more than an alternative for amputation. "Do not the very circumstances," observes Dr. Watson, of Glasgow, "which demand the amputation, diminish the likelihood of success from excision? Surely, then, the two operations ought not to be canvassed at the same time. If it is the time for amputation, then it is too late to attempt the salvation of the limb by excision; and if the latter course is at all a desirable one, then ought we to pursue it before the patient's case has become well-nigh desperate. I believe it has been by acting too rigidly on the opinion that excision was a mere substitute for amputation, that we have had such poor success with our cases in Glasgow, and if I am not very much misinformed, surgeons elsewhere have not so acted, but have chosen cases for excision that were by no means *ready* for amputation. I think that we should do so too—that we should select patients who have some health and strength to work upon, as the subjects of this operation, and not those who are already sinking into the grave."

In concluding this brief record, I should be failing in duty were I to omit expressing my warm thanks to Messrs. Cooke, Locke, and Crosslé, our clinical assistants in the surgical wards of the hospital, for their care, and untiring zeal and energy, not only during the progress of the case I have just recorded, but in all those they had under their care since they were appointed to the offices they recently so worthily occupied.

IV. *Resection of Metatarsal Bones.*—I shall conclude this clinical report on the resection of joints and bones, by briefly adverting to a case of very chronic strumous disease of the metatarsal bones, which was under my care last September, and for

which the operation of resection was attended with the happiest results. The following are the leading particulars of this case:—Matthew Davis, aged sixteen, a boy of very strumous habit, was admitted into the Meath Hospital on the 20th of last September. He stated that about two years previously he suffered from the formation of an abscess, which he could not ascribe to any cause, never having received any injury whatever to the foot. At the time of his admission he suffered greatly from pain. At points corresponding to about the centre of the third and fourth metatarsal bones were situated sinuses, through which oozed a thin, sanious, unhealthy-looking discharge, and on introducing a probe through them, softened, denuded bone could readily be felt. There was a good deal of general infiltration and swelling about the diseased structures. In consequence of this condition of the patient's foot, he was quite unable to follow any occupation. Various lines of treatment had been had recourse to previous to his admission into the Meath Hospital, but as yet all treatment had proved unavailing. My colleagues, Messrs. Porter and Collis, quite agreed with me in thinking that as the disease seemed so localized, an attempt to preserve the anterior portion of the foot, by performing a resection of those metatarsal bones in which the disease appeared, should be made. Accordingly on the 27th September I performed the resection in the following manner:—I commenced by making an incision about two inches and a-half in length on the situation of the fourth metatarsal bone, and at the centre of this I made another an inch and a-half in length, at right angles with the first passing inwards. The two triangular-shaped flaps thus made were dissected back, and soon we were enabled to see the diseased osseous structures. The periosteum was very much thickened, and consequently easily detached from the bones, which were, without much difficulty, removed by the aid of resection saws, gouges, both straight and rectangular, and sequestrum forceps, &c. The diseased bones being removed, the edges of the wound were brought into close apposition by means of silver points of interrupted sutures, dry lint dressings were applied, and the patient sent to bed. It is unnecessary to detail the daily account of the progress of this case. In the course of ten days the wound was so nearly entirely healed that all dressings were removed, and about a fortnight after this the wound and the sinuses being completely healed, the patient, free from pain, left the hospital, enabled to walk about without any distress or inconvenience whatever.

ART. II.—*The Spontaneous Elimination of Uterine Tumours.* By ALFRED H. M'CLINTOCK, M.D.; F.R.C.S.I.; late Master of the Lying-in Hospital, &c.[a]

IN this communication I propose considering the modes by which nature occasionally relieves the uterus of the presence of a solid growth. The very great frequency of such growths in this organ, and the serious consequences which they so constantly entail, invest their history with deep interest for the practical gynecologist.

To study any disease aright we should commence by studying the course which it takes when left to itself, and uninfluenced by the active interposition of art. Of such an investigation there is no more important branch than that which relates to the conservative processes whereby nature removes from the system morbid products constituting the essence or the effects of a disease. This paper I would offer, then, as a contribution towards the natural history of solid tumours of the uterus—an attempt to improve and extend our knowledge concerning the spontaneous elimination of these neoplasms.

From the title of this memoir it will be seen that I do not strictly confine my observations to any one class of tumours, though, in point of fact, the tumours to which it relates were almost entirely of the fibroid or fibro-sarcomatous kind, the occurrence of any other kind being extremely rare in this organ.

Out of all cases of tumour of the uterus, the proportion in which a curative process is set up is comparatively but very small, whilst the number in which nature effects a cure is still smaller. Nevertheless, many cases of spontaneous elimination are recorded, and there are few obstetricians of any considerable experience who have not met with two or more instances. An acquaintance with this subject is, therefore, absolutely necessary, if we would aim at correct diagnosis, or avoid committing gross errors.

It is to be remembered that the processes set up by nature for the destruction of these growths, although conservative in their tendency, sometimes involve considerable danger to life.

The study of a large number of recorded histories, together with the results of my own clinical observation, lead me to believe that there are *five* different modes by which nature may effect the cure

[a] Read before the Dublin Obstetrical Society.

of a tumour of the womb. To reduce the number cannot be done, I think, without confounding cases essentially different, whilst to increase it beyond five would be devoid of any real utility. These five modes of elimination are as follows, viz.:—

1. Interstitial absorption.
2. Simple detachment, or separation.
3. Calcareous transformation, or petrifaction.
4. Sloughing, or disintegration.
5. Expulsion by uterine contractions.

I have arranged them in this order, believing that it represents—approximately at all events—the relative degree of risk attaching to each process, absorption being the least dangerous, expulsion the most so. Were the cases to be arranged according to frequency, the order would be somewhat different. *First* and most frequent would be the cases of sloughing; then the cases of detachment; after them the cases of expulsion; next those of calcareous degeneration; and lastly, those of interstitial absorption. I do not pretend to affirm that this order is strictly correct, for it is not deduced from any minute statistical calculation; still I think it will be found pretty near to the truth.

1. The *absorption* of a solid uterine tumour, even with the aid of medicine and other agencies likely to favour the process, is considered by some an impossibility. It is true, as a general rule, that heterologous formations are not removable by absorption. But is it correct to regard fibrous tumour of the uterus as such? Upon this point I can cite no higher authority than that of Virchow:—"We find, for example," he says, "that the so extremely common form of uterine tumour, which has been designated fibrous or fibroid, has in every respect the same structure that the walls of the 'hypertrophied' uterus have, inasmuch as it consists not only of fibrous connective tissue and vessels, but also of muscular fibre cells. The tumour may, as is well known, become so large as not only to embarrass the uterus in all its functions in an extreme degree, but also to exercise, through pressure, the most injurious influence upon the neighbouring parts. In spite of this, it must always be considered an homologous structure."—*Cellular Pathology*, p. 443.

But again, in cases where a fibrous tumour (or what was supposed to be such) was absorbed, the correctness of the diagnosis has been disputed, and it is urged that the tumour was only an inflammatory

or œdematous swelling, or a chronic hypertrophy, or some tumefaction whose removal we know to be quite probable. Objections of this kind may no doubt be advanced, and the possibility of an error of diagnosis in any particular instance, can very seldom be completely excluded. Still there remains an amount of evidence on this question, from which we cannot escape drawing the conclusion, that nature is capable, on some rare occasions, of entirely removing a solid—it is to be presumed a fibrous—growth by the process of interstitial absorption. This position derives corroboration from the cases—and they are not a few—in which the same result was brought about by the use of therapeutic agents. One such case occurred to myself, and has been published.[a] I do not here refer merely to cases—common enough—in which the tumour undergoes an apparent reduction of its bulk after treatment, menstruation, or delivery, and which reduction is to be explained by the removal of congestion or œdema. I am speaking now of the actual absorption of the growth itself.

A very striking example of the complete removal of a fibrous tumour by absorption is recorded by Dr. Matthews Duncan, and what enhances the value of this case (besides the thorough competency of the observer) is Dr. Duncan's own acknowledgment that he has been, as it were, forced against his judgment, by the evidence of a single case, to admit the possibility of the complete removal of a large fibrous tumour by absorption. He writes:—"The tumour was as large as the fetal head at the end of pregnancy. It was as easily and as perfectly diagnosed as any case could be. There was no doubt ever thrown upon the nature of the case by any of the experienced practitioners who examined it. It had every character and symptom of a fibrous tumour. The patient was long in the most aggravated state of anemia. *Now*, there is as certainly no uterine tumour, as there was certainly one formerly. The only method of escape for me, in the evidence of this case in favour of complete absorption, is the supposition that the tumour may have become spontaneously enucleated, separated and discharged, without the consciousness of the patient. This alternative, I confess, considering the cleanly habits and truthful character of my patient, seems more unlikely than the other."

Dr. Routh tells us in his Lettsomian Lectures that he "has met with at least two distinct cases of large fibroid, which, he should say, filled the pelvis, and materially interfered with the functions

[a] Clinical Memoirs on Diseases of Women, p. 141.

in that cavity, where the tumours have gradually diminished to the size of small apples" (p. 38).

It is worthy of notice, and Dr. Routh pointedly alludes to the fact, that in both these instances, as well as in others, the atrophy of the tumour took place contemporaneously with the change of life, which would seem to imply that it resulted from arrested nutrition.

Whether the structure of the tumour undergoes any degree of softening preparatory to its removal by the absorbents, it is impossible to say. Dr. West thinks that it does, and a case recorded by the late Dr. Rigby rather supports this view, as he states, in the course of its history, that the tumour became *softer* and smaller.

The processes of transformation and absorption going forward with such activity in the uterus after parturition may, perhaps, be extended to a growth standing in close vital relation to the organ, and thus lead to its partial or complete dispersion. Although parturition is likely to produce in the tumour other effects than its simple absorption, still experience shows that this latter is an occasional consequence, and the most fortunate one for the patient. "If," says Scanzoni, "we consider the great vascularity, the hyperemia, the infiltration, and the softening which these tumours present during gestation, it will be seen that if there is any time at which the conditions are favourable to absorption it surely is the puerperal state" (p. 237 of American edition). The same high authority reports a case where "a fibrous body of the size of a man's hand, the diagnosis of which was perfectly sure, disappeared during confinement, in a manner so complete that six weeks after parturition we could no longer discern a trace of this tumour, which had existed for eleven years" (*op. cit.*)

2. *Detachment.*—From the meaning of the term detachment (or severance), it is obvious that only such tumours as are pediculated—polypi in fact—can be the subjects of this mode of elimination; but it matters not whether they spring from the mucous or the serous surface of the womb. There are three ways in which this separation may be effected, and numerous examples of each could be adduced. First, the pedicle may give way from simple atrophy and attenuation; second, when the pedicle is small it may break or snap asunder in the act of extrusion from the uterine cavity, or from the weight of the suspended tumour; and lastly, the pedicle may be destroyed through the constriction exerted

upon it by the os uteri or ostium vaginæ. If the detached tumour remain in the genital canal, it will, of course, speedily decompose and become putrid.

Case I.—In the year 1863 Mr. Richardson asked me to see with him a patient from Drogheda, aged forty-two, who had some uterine symptoms. On examination we found in the vagina a polypus the size of a wallnut, in a completely sphacelated state. It had grown from the interior of the womb, and the small stalk had apparently been strangulated by the grasp of the os uteri. The shred of decayed pedicle which remained was broken by a slight effort of torsion.

Should a polypus become detached whilst still within the uterus—a thing not wholly impossible, perhaps—and be retained there till decomposition sets in, it would be very hard to distinguish such a case from one of those to be presently described, in which sloughing or disintegration of a tumour occurs as the primary lesion. This obscurity would have hung over the following case, had I not been made acquainted with the previous history of the patient.

Case II.—In the month of April, 1859, Dr. Fitzpatrick, then medical attendant of the Moynalty Dispensary, County Meath, but now of Sussex Gardens, London, sent up to the Lying-in Hospital (of which I was at the time Master) a patient, in whom he had ascertained the existence of a smooth pyriform polypus, insensible to the touch, and encircled above, at its neck, by the os uteri. Some days intervened between the dates of his examination and of her arrival at the hospital. I found the uterus low, its orifice considerably dilated, and a firm rough tumour within the os, and partially protruding from it. There was an exceedingly fetid discharge. The surface of the tumour was seen to be of a dark green and brownish colour. I took hold of this tumour with a vulsellum, but on pulling firmly it gave way, and only a small bit was removed, which was evidently in a gangrenous state; but its fibrous structure was still apparent, and it lacerated in a definite direction. In the course of the succeeding week I extracted several fragments of the tumour in a decomposed state, and a good deal more came away in the discharge. During this period her life was seriously threatened, from the amount of irritative fever which was present. She had rigors; a small, rapid pulse; dry tongue; total loss of rest and of appetite. According as portions of the tumour,

day by day, were withdrawn, the os began to close, and the constitutional disturbance to subside, so that by the end of the month she was able to leave the hospital and return home. I should mention that she was a widow, aged sixty, and had borne nine children. For upwards of two years she had been subject to constant hemorrhages, but these had entirely ceased a short time before she came to the hospital, in consequence, we may presume, of the strangulation of the tumour. The *rationale* of this case is plain enough, I think. The polypus was expelled through the os, whilst the pedicle was still insufficiently long for this change of place. Complete strangulation of the tumour now took place, entirely destroying its vitality. Consequent upon this, it diminished in size, so as to permit of retraction within the uterine cavity; and such was the state of things when the woman came under my care.[a]

Polypoid fibrous tumours, growing by a slender pedicle from the peritoneal surface of the uterus, may become detached by some sudden movement of the body, whereupon the tumour drops into the cavity of the belly; of which Simpson states that he "has seen several instances" (*Obstetric Works*, Vol. i., p. 117), and Dr. Turner has very fully reported another (*Ed. Med. Jour.*, January, 1861). In one sense the tumour is now to be regarded as a foreign body lying within the peritoneal sac, and so far is analogous to the loose cartilaginous bodies found in the knee-joint. It would appear from the reports of cases that the presence of a loose fibroid in the belly is perfectly innocuous, and does not ordinarily excite any irritation in the contiguous viscera or serous membrane. The tumour itself undergoes no change, but presents the same appearance as these tumours are wont to do when still in vital connexion with the womb.

It not uncommonly happens that before its separation the tumour has contracted an adhesion to some adjacent part, and when so circumstanced its severance from the uterus is more apt to occur, especially if the new connexion be to a viscus, such as the bladder or rectum, which is constantly undergoing changes in size and position. Simpson reports the case of a lady who, from her unusual shape after delivery, was supposed to have an extra-uterine fetation. She died of puerperal peritonitis in the second week, and a fibrous tumour was found adhering firmly to the peritoneum lining the anterior wall of the abdomen, while its pedicular

[a] The full particulars of this case will be found at page 166 of my Clinical Memoirs on Diseases of Women.

attachment had stretched and lacerated during the involution of the organ after delivery (*op. cit.*, 834).

3. *Calcification.*—A much rarer event than either of those we have been considering, is the transformation of a fibrous tumour into a calcareous mass which ceases to grow, and may be partly or entirely discharged. It is a degeneration from animal to inorganic substance. The propriety of regarding this change as a mode of elimination may be disputed. The tumour certainly remains in most cases, but it ceases to hold a vital connexion with the economy, and the symptoms which it had previously produced abate or altogether disappear.

In a very few instances the structure of these calcified or petrified fibroids has a near resemblance to bone, but such cases are quite exceptional. In the vast majority no true bone is formed, but earthy particles, chiefly salts of lime, "are deposited and accumulated in the organic basis of the tumour, so as to make a mass which is sometimes friable and porous, sometimes hard and dense as marble" (Simon). By what process this remarkable conversion or transformation is brought about we cannot tell. Inflammatory action would seem to have no part in it. The appearance of the tumour and of its nidus indicates this. Nay, more, guided by the analogy of the cretaceous transformation of tubercle, and of the so-called ossification of arteries in the aged, we should rather regard it as the result of defective nutrition. "This much at least can, with certainty, be affirmed (writes Virchow, p. 365), that we are as yet acquainted with no stage in these changes which is at all akin to inflammation." Although we must confess our ignorance of the precise way or mode by which this calcification, or, to use the term proposed by Virchow, "petrifaction," of a fibroid is produced, still experience has shown us the circumstances and conditions which favour its production. It is chiefly met with in women advanced in life. It is confined nearly altogether to subperitoneal, or extra-mural fibroids; and generally to such as are pediculated, but not invariably so, as Dr. Turner would lead us to suppose. In the Museum of the Royal College of Surgeons (Ireland) is a very fine preparation, presented by the late Sir Philip Crampton, which strikingly exemplifies these points. The patient was an elderly woman, who married late in life, and never bore children. The disease was supposed during life to be ovarian dropsy, and did not appear to be the cause of death. The uterus contains several fibroids, the largest of which is about the size of a fetal head, and

has the hardness and density of bone. This tumour lies almost immediately under the peritoneum, but is still imbedded in the uterus, and not in the slightest degree pediculated. In direct apposition with it is a large interstitial fibrous tumour, which contains merely a trace of calcareous deposit.

Again we find that this process of petrifaction may be limited to the surface of the tumour, or pervade its structure. It "has no necessary connexion with the size of the tumour, and is not commensurate with its growth; but it appears (says Dr. Turner) to be in intimate relation to the size of the peduncle, and to the changes which take place, by obliteration or atrophy, of the vessels which pass from the substance of the uterus through the peduncle to the tumour."

A tumour that has undergone this calcareous transformation ceases to be productive of symptoms, except such as may arise from its mechanical influence. On rare occasions the calcified tumour has fallen from its nidus, and been discharged *per vaginam*.

It does not always prove quite so innocuous, however. Cases have been reported in which these petrifactions, by infringing on a neighbouring organ, have caused ulceration and other ill effects. Dr. Turner cites a case where a large calcified tumour, growing from the back of the uterus, caused death by compression and rupture of a fold of the ileum, consequent upon a fall on the pavement.

Dr. Matthews Duncan exhibited, at the Edinburgh Obstetric Society, a calcified fibrous tumour of the uterus, from a patient who had died of peritonitis, and two holes were found in the peritoneum, apparently made by the tilting upwards of two thick scales of the tumour.

Through the kindness of Dr. Fleming, I once had an opportunity of seeing a most singular—indeed, I may say, unique case, in which a calcified tumour, springing from the anterior wall of the uterus, had made its way by ulceration into the bladder, producing all the symptoms of vesical calculus in their most intensely aggravated form. Some fragments of the tumour had been detached, and were found in the cavity of the bladder.

4. *Disintegration or Sloughing* is another change that may take place in a uterine tumour, and lead to its partial or complete extirpation. This process generally gives rise to symptoms of a grave kind, and in many instances the patient has actually sunk under the wasting effects of the discharge and the constitutional irritation.

Suppuration of fibrous tumours is described by some authors, but I see no necessity for making a separate category of such cases. In many of them the discharge was plainly due to softening of the tumour and irritation of the uterine cavity.

The vitality of the tumour having been destroyed—how, we shall presently inquire—it may come away *en masse*, or in broken, irregular fragments, quite putrid, at intervals of days, weeks, or months. All this while there is little or no pain, but a constant puriform discharge of a highly offensive character, and sometimes mixed with blood, is going on. In a few instances the tumour is too dense to melt away, and too large to be expelled spontaneously. Here art must interpose, or the life of the patient will inevitably be lost. Dr. Hall Davis met with two cases in the Middlesex Hospital, which strikingly exemplify the good effects of this timely interference. Very alarming symptoms, viz., fever, delirium, vomiting, &c., were present in each case; but the lives of both patients were saved by dilating the os, and removing the putrid *debris* of the tumours. I may here venture on observing that it was the study of such cases as these that led me soon after its first announcement, to form an unfavourable opinion of the operation of gouging uterine fibroids.

I have said that the coming away of the tumour may extend over a period of many weeks or months. A case will be presently related, in which six weeks were so occupied; and Dr. Churchill has kindly furnished me with the outline of a case where the process extended over a still longer period.

Case III.—A widow lady, under fifty years of age, had uterine hemorrhages for a long time. Three tumours were ascertained to be present in the uterus, and could even be readily distinguished by external examination. Two of these tumours sloughed, one after the other, and came away per vaginam, at different times, in a softened, putrid state. From first to last the eliminative process occupied several weeks. During this time there was a profuse and very fetid discharge from the vagina; and there was much constitutional disturbance, but no uterine pain. After the second fibroid had passed away she recovered her health. The tumour remaining in the uterus could now be felt by itself. Soon after this the lady died of cholera; had she lived, it is probable the remaining tumour would have been cast off.

A very interesting but difficult question presents itself for solution,

with regard to the cases comprehended in this class, viz., what causes the sphacelation or death of the tumour? Both Dr. West and Dr. M. Duncan maintain that inflammation has nothing to do with it; whilst Cruveilhier, Scanzoni, and Simpson hold an opposite opinion. I am at a loss how to reconcile this latter view with the absence of all local symptoms of inflammation in or around the tumour. No doubt inflammation may arise in the progress of a case, but then it is a consequence, an effect, not the cause, of the death of the tumour: and further, when it does supervene under these circumstances, it is generally peritoneal or phlebitic.

There is yet another way in which, I think, we may very satisfactorily account for the death of the tumour, independently altogether of the influence of inflammatory action, namely, as the result of spontaneous enucleation, whereby its structural connexion with the uterine tissue becomes so limited as to be inadequate to the continued nutrition of the tumour, and, as a consequence, its vitality ceases—it dies, in short.

I am not aware of any fact directly confirming this supposition; but many cases have been recorded—especially by Dr. Matthews Duncan and by Cruveilhier—tending to show that it is not only possible, but highly probable; and so far, the explanation has more to commend it than any other theory that has been put forward to account for the spontaneous dissolution of an intra-uterine tumour.

An admirable example of the particular mode of elimination we are now considering lately fell under my notice, and I trust no apology is needed for presenting an outline of its history.

Case IV.—The subject of the case was a large corpulent widow lady, aged sixty-eight, the mother of twelve children. She first consulted me in June, 1864, on account of recurring attacks of metrorrhagia, to which she had been liable for six or eight years. After due investigation I made the diagnosis of a large fibroid to the left side of the uterus. This accorded with opinions previously given by the late Professor Montgomery, the late Dr. Charles Johnson, and by Dr. Beatty.

I did not again see this lady till the latter end of last April. She then told me that for some months there had been little or no bleeding; but, that during the three or four weeks preceding my visit, she had suffered extreme annoyance in consequence of a profuse watery discharge from the vagina, of an intolerably fetid odour. Since the occurrence of this discharge the following symptoms had

developed themselves, viz., total disinclination to food; occasional attacks of diarrhea and sickness of stomach; severe rigors on three or four occasions; sleeplessness; a frequent pulse; foul tongue; and œdema of the feet and ankles. The os uteri was healthy, but dilated to the size of sixpence, and within it could be seen a portion of substance, evidently sloughy, which I endeavoured unsuccessfully to remove with a dressing forceps. This discovery led me to suspect the nature of the case, and I explained my views to the late Dr. Banon, who, as the family physician, saw this lady occasionally with me. At next visit (two days afterwards) she complained of a feeling of pressure on the rectum, and of dysuria, when I found a very large, softened tumour filling the whole vagina. The relations of the upper part of this decomposing mass I could not satisfactorily ascertain, in consequence of its enormous size, and the tenderness of the vagina. She had experienced no particular sensation that would have marked the time when the tumour passed out of the uterus; and I may here take occasion to state that at no time, from the beginning of present symptoms, was there any abdominal pain or tenderness whatsoever. Assisted by Dr. Banon, I fixed the points of a strong vulsellum in the tumour, and began rotating it. At first there was some little resistance to the movement, but this soon ceased, and the tumour could be turned round and round—in fact, it was quite free. We now set about extracting it, but our united efforts were required to get it through the vulva; at length it came away with a violent jerk, and was followed by the escape of a large quantity of abominably offensive purulent fluid.

The tumour had the size of a large cocoa nut, but was soft and sloughy. It was in a highly putrid state. The preparation of this tumour is now on the table, and belongs to the Museum of the Royal College of Surgeons. Dr. John Barker was kind enough to examine a section of it under the microscope, and he reports that he "could detect in it no specific characters of a malignant nature: it was very fibrous, with a large quantity of elastic tissue pervading its structure."

From the time the tumour came away all this lady's symptoms began to subside, so that at the end of a fortnight not one remained. She soon afterwards returned to the country in better health and spirits than she had been for many years.

On many occasions the death and discharge of tumours has followed parturition, either from the effects of some injury which it sustained during the process of labour, or from the vascular supply

to the growth, being diminished by the contraction and subsequent involution of the uterus. Where this dissolution of the tumour has taken place at an early period in childbed, the consequences have generally been fatal to the patient.

Dr. Gardener, the American editor of Scanzoni, suspects that some of the pelvic cellular abscesses occurring after labour may, perhaps, be properly considered as the result of the breaking down of previously unsuspected fibrous tumours; and he adds, that several cases he has seen, while they are not perfectly convincing, force him to give this opinion some consideration. Pelvic abscess is known to be an occasional complication of uterine fibroids. In some of these cases it may be that the abscess depends on the disintegration of the fibroid, as Dr. Gardener describes. Certainly one case of this kind fell under my notice some years ago.

Case V.—The patient was a middle-aged married woman, never pregnant, who had a large fibroid in the posterior wall of the uterus, for which she had been under my care for nearly three years. An abscess formed behind the uterus, and burst into the rectum. It continued discharging for some months, during which time the tumour underwent a gradual diminution in size, till it was reduced to about one-fourth of its original bulk. Eventually, however, the patient—who was very much anemiated, and had always been of a very delicate and highly scrofulous constitution—sank under the discharge and constant irritation of the rectum.

5. *Expulsion.*—The fifth and last mode of elimination I term expulsion, under which title are comprehended all those cases where uterine contractions—parturient efforts, in fact—form the leading symptom as well as the efficient cause of the separation of the tumour, which is still in vital connexion with the womb when the contractions commence. In the group of cases last considered, expulsive pains were very rarely present, and when they did occur, it was *in consequence of the death of the tumour*, which then acted as a foreign body, and excited parturient efforts on the part of the womb.

As the uterus is endowed with very great contractile power, we might, *a priori*, expect solid growths to be got rid of occasionally by the exercise of this faculty. Experience amply confirms this, but at the same time teaches us what we otherwise should not be quite so prepared for, namely, that the danger arising from this mode of elimination is greater, perhaps, than from any of the preceding.

A good many cases are reported where this species of parturition has been set up, and the tumour has thereby been partially or entirely removed. A large proportion of these cases occurred in the puerperal state, which may go far to account for the large mortality amongst them. But setting aside the puerperal cases, the danger would seem to be greater from *expulsion* than from *sloughing*. This more unfavourable result admits, perhaps, of some explanation. The expulsive process is the more acute: it is often accompanied by prolonged pains of a very severe kind; and lastly, the violent contractions of the diseased organ must powerfully tend to excite inflammation, or other morbid action in it.

Let me now submit the sketch of a very apposite case.

Case VI.—In July, 1864, I met Dr. Bernard, of Dundrum, in consultation upon the case of a widow lady, aged forty, who four years previously had begun to lose blood in large quantity from the uterus. For the last twelve months, however, there had been no hemorrhage of any serious kind, and the catamenia were moderate. The uterine tumour reached as high as the umbilicus. In the lower part of it a short, loud souffle was audible.

The patient was a small, delicate woman, very ex-sanguine. At the time of my visit, and for some hours previously, she had been complaining of pain, of an intermitting kind, in the tumour, and there was a good deal of watery discharge. I found the os uteri dilated to the size of a shilling; its edges very thin, and the tumour pressing firmly into it. Some days later regular labour pains, of a very violent and expulsive character, set in. When I saw her they had continued for six hours without any abatement, and she was much exhausted, and her distress and weakness were aggravated by a continual nausea or vomiting. The abdominal tumour had undergone a considerable reduction in its size; whilst the vagina was distended down to the perineum with a fleshy mass, rendering it necessary to draw off the urine with the catheter. By the aid of morphia she got a little respite from the severe bearing down pains, but nothing stopped the sickness of stomach. Some hours later I found the tumour protruding from the vagina. Pains were still present, but she was altogether very low and weak, and harassed by incessant vomiting. I succeeded in removing a portion, the size of a large orange, of the tumour. It was of a dark red colour, with a fleshy structure, but admitting of being lacerated with the fingers, in which manner I detached it. No more could be got

away, and she died a few days afterwards, utterly worn out by pain, hemorrhage, vomiting, and loss of sleep. I much regret an autopsy could not be obtained.

In some respects, though certainly not as regards its issue, this case bears a resemblance to one of Dr. Rigby's, in which, by successive operations, he removed 35 lbs. of tumour, which was gradually forced down by regular labour pains.

Dr. Thompson, of Omagh, has furnished me with the particulars of a case, in which the successful removal of a uterine tumour, weighing nearly seven pounds, was effected through the influence of parturient efforts, seconded by surgical treatment.

Case VII.—The patient was a married woman, aged thirty, who never had borne children. When she came under Dr. Thompson's care she presented all the symptoms of extreme anemia. She was also very weak, and incapable of the least exertion, and for some days previously had retention of urine. On examination he found an enormous tumour filling the vagina, and distending the womb above pubes to the size of a five months' pregnancy. This tumour was so firmly jammed in the pelvis that it was impossible to reach even its greatest circumference. Its surface was smooth, pale, and traversed with large veins. It was not sensitive or painful, though its pressure on the rectum and urethra was productive of much distress.

The bladder was emptied, and an attempt made to pass a ligature round the upper part of the growth, but from the want of a proper instrument, this failed. In the course of the next few days the tumour began to protrude beyond the vulva, and to assume a gangrenous appearance, being flaccid, dark coloured, and fetid. As the fingers could now be passed up the vagina, and beside the tumour, with less difficulty than before, Dr. Thompson applied a tight ligature round the tumour as high up as he could reach, but not to its neck, and ordered a mixture of biborate of soda and strong infusion of ergot of rye. This acted in a most marked and decided manner on the uterus, producing pains similar to those of labour, and unlike anything she had before, and with the effect of bringing down the tumour so low as to expose the ligature. The pains continued until the whole tumour—which nearly equalled a mature fetus in bulk—was expelled, and hanging by a pedicle of about an inch in diameter. By this time she was reduced to a state of alarming prostration, and she was only kept alive by

the unremitting administration of brandy, &c. On the following day, as her condition was very little better, Dr. Thompson determined on the excision of the tumour, and this he effected with the ecraseur, having previously tied the pedicle as tightly as he could, to guard against any hemorrhage. This precaution was very needful, as there were found to be some large vessels traversing the pedicle. The tumour weighed 6½ lbs., and was of a fibro-cellular structure. The patient slowly rallied out of this state of extreme depression, and recovered without a bad symptom.

I would wish to draw attention to two points in the foregoing history: first, that the expulsive contractions of the uterus (without which the removal of the tumour could not, in all probability, have been effected) were artificially induced; and second, that the tumour was apparently of a polypoid nature. Neither—nor both—of these circumstances, however, exclude the case from the class now under consideration.

The following case was communicated to me. It very well exemplifies the danger attendant upon a long continuance of uterine efforts to get rid of a morbid growth.

Case VIII.—The patient's age was forty-five; though married for twenty years she had never been pregnant. For many months she had been losing large quantities of blood from the uterus, so that she was extremely weak, and anemic to the highest degree. The uterus was enlarged to the size of pregnancy at five months, from the presence of a hard globular tumour, which could be felt through the thin and partially-open os uteri. Latterly she had severe pains in the womb at each menstrual period, and to relieve these she was in the habit of taking opiates pretty freely. To aid in the expulsion of the tumour she was ordered to take at next period one drachm of ergot, infused in a wine glass of hot water, three times a-day; this direction she followed.

Strong uterine action supervened, which so far expelled the tumour that its uterine attachment, which was very extensive, could be reached. No artificial means to detach the tumour, however, were tried, and the woman sank in the course of some days, apparently worn out by the protracted and unavailing uterine efforts.

The similarity between this and Case VI. is very great. They both strongly suggest the propriety of attempting the removal of the tumour by artificial means, under circumstances such as those

described. Gooch relates the history of a case closely resembling these; it occurred in the practice of Mr. Barrett, of Yarmouth. The patient sank under the long-continued fruitless expulsive pains, when a tumour weighing nearly four pounds was found growing by a thick stalk from the os uteri. Had this tumour been excised, as was done in Case VII., the woman's life might, in all probability, have been saved.

It has been already stated that the recorded instances are numerous in which expulsion of a tumour occurred as a sequel of childbirth, and that the danger of the complication was in proportion to its earliness after delivery. Time will not permit of allusion to any of the published cases which might very profitably be adduced to illustrate this part of the subject. One remarkable case I must mention, where the expulsion of a tumour, weighing nearly four pounds, followed six days after a three months' abortion, the patient recovering perfectly, and giving birth to a healthy, mature child, fifteen months subsequently. This remarkable case occurred in the practice of the late Dr. J. Beatty.

I must now bring this very imperfect memoir to a close. I purposely say imperfect, because I have not fully entered on the question of treatment; and also because little or no reference has been made to the researches of others on this subject.[a] In fact, the observations are based mainly on the results of my own experience.

ART. III.—*Introductory Address at the Opening of the Winter Session of the Medical Society of the College of Physicians.* By FLEETWOOD CHURCHILL, M.D., President of the King and Queen's College of Physicians.

GENTLEMEN,

By the favour of the College of Physicians, it is my privilege to occupy this chair, and it becomes my duty to make a few observations, introductory to the opening of the session of the Medical Society.

I am not ashamed to confess that I feel it a great pleasure to be so honoured, and not the less so that I cannot plead a consciousness of desert.

[a] Dr. C. West is almost the only English writer who has treated this subject at any length.

Yet, and I trust you will excuse the avowal, the pleasure, like other pleasures, is not unmixed with sadness when I look round and see how few of my compeers of former years remain; how many of those are gone whose energies in the pursuit of truth, and whose great attainments, constituted so much of the value and ornament of the Society and its predecessor.

They have their "quiet consummation," and we "have entered into their labours," and in so doing we incur the responsibility of carrying on our science from the point where they left it, to the uttermost that our lives and abilities will permit.

The record of their labours is in itself the best commentary upon the value of such Societies as this, and may well spare you the trouble of listening to a more elaborate defence of them.

Perhaps, however, a few words upon one aspect of such Societies—I mean as Educational Institutions—may not be out of place, and it will not detain us long.

No greater mistake could be made, nor one fraught with more disastrous consequences, than for any one to conclude that he has finished his education when he has taken his degree and passed all his examinations. It is, in truth, just then that his true medical education begins. If we are to do any good in our day and generation; if we are to leave our science more advanced than we found it, we must be students to the end, feeling how little we know, how liable we are to prejudice, self-deceit, and error; and needing, therefore, in our daily struggle after knowledge, all the help we can obtain, and, above all, such help as these Societies afford.

I think I may assume that all, or nearly all, great discoveries, all true and extensive generalizations, have been the result of solitary labour and study. But being so, they have the unavoidable drawback of being more or less one-sided.

We are all, naturally perhaps, but unduly prejudiced in favour of the theory we have adopted, or the conclusions we have drawn from our observations, or the solution of any problem we have worked out; contrary arguments come with diminished force, and objections, being unwelcome, are ignored or lightly valued.

Probably the very best counteractive to this danger is a Society like this, where each has to submit his productions to the scrutiny of his fellows; to a discussion where no mercy will be shown to weak reasoning, careless observation, or defective generalization; but all justice done to care, and thought, and accuracy.

Any system, any theory, any supposed discovery that will not

stand such a test may be summarily dismissed; but in proportion as it involves truth, it is sure to stand; not, perhaps, exactly as the author moulded it; nor with its first beauty of form and completeness of detail, but shaped and moulded, by the larger experience and more matured judgment of others, into something far more valuable. The aggregate of facts contributed by the experience of such a Society may render a theory untenable, but it will pave the way for a wider and more correct generalization.

There is, however, one condition, without which we can as little obtain the full benefit of public discussion as of private research, and that is, that each for himself should aim at the discovery of truth for its own sake, and from no lower motive. Truth, I mean, whether it tells for or against our own views; truth, notwithstanding, it may be, the prejudices of education, or of partial training, or of limited experience. If we would be good or do good, truth always and by all means must be our supreme aim in science as well as in morals or religion.

If you permit me I will read you an extract from the inaugural address delivered at Glasgow by John Stuart Mill, embodying, in eloquent words, profound philosophic truth.

He says:—"The most incessant occupation of the human intellect through life is the ascertainment of truth. We are always needing to know what is actually true about something or other. It is not given to us all to discover great general truths, that are a light to all men and to future generations; though with a better general education the number of those who could do so would be far greater than it is. And the need we have of knowing how to discriminate is not confined to the larger truths. All through life it is our most pressing intent to find out the truth about all matters we are concerned with. Every time we have to make a new resolution, or to alter an old one, in any situation in life, we shall go wrong, unless we know the truth about the facts upon which our resolution depends. Now, however different the searches for truth may look, and however unlike they really are in their subject matter, the methods of getting at truth and the tests of truth are in all cases the same. There are but two roads by which truth can be discovered—observation and reasoning—observation, of course, including experiments. We all observe, and we all reason; and therefore, more or less successfully, we all ascertain truths; but most of us do it very ill, and could not get on at all, were we not able to fall back upon others who do it better."—pp. 2–12.

No doubt much of our observation is incomplete and inaccurate, and much of our reasoning specious rather than true; but if there be one feeling more than another calculated to supply the necessary caution and care, and to induce us to undergo the labour which the pursuit involves, I believe it to be the consciousness that our object is solely and simply the discovery of truth. And if we each bring this principle into exercise in our discussions as well as in our private pursuits, then, I think, no one will question the value of such Societies as Educational Institutions.

Any one who attended the meetings of this Society last winter must have been satisfied, not only that a number of valuable facts were accurately observed and reported, and inferences very cautiously deduced, but that the various communications and the discussions brought out very prominently that the discovery of the truth was the one object of all concerned. I can do no more than refer you to the great debates upon cholera and purpuric fever, or Black Death; the most interesting I have ever heard here or elsewhere, and displaying a vast range of experience, with the most scrupulous care that the deductions should only cover the facts.

The following list of names mentions the papers read during the past session, but it does no more, nor have I time to supply the deficiency by an analysis:—

Dr. Stokes, President of the College of Physicians, opened the session with an Address.

Rev. Professor Haughton read a "Scientific Inquiry into some of the Cases alleged to produce Cholera."

Dr. Henry Kennedy—"Some further Observations on Hydrocephalus and its Treatment."

Dr. Grimshaw—"On the Sphygmograph."

Dr. Lyons—"On Abscess of the Liver and the question of puncturing it." Also a "Case of Facial Paralysis of a peculiar form."

Dr. Hughes exhibited a boy who had been affected with pemphigus to an extraordinary extent. He also related a remarkable case of Paralysis.

Dr. Foot—"On the Form of Depraved Appetite, known by the name of 'Pica.'"

Dr. Moore Madden—"On Dreaming, considered specially in reference to Insanity."

Dr. Marcus Eustace—"Symptoms diagnostic of Recovery in the different forms of Insanity."

Dr. Head—"Notes of a Case of Fatty Degeneration of the Diaphragm."

Dr. Byrne—"A Case of Abortion at five months, apparently depending on Choleraic Poisoning."

Dr. P. C. Smyly—"Some Cases of Paralysis following Blood-poisoning."

And in addition, as I have said, the pathology and treatment of cholera, based upon reports from different hospitals, occupied the Society on three special evenings, as did also the debate upon the malignant purpuric fever.

I cannot conclude these observations, gentlemen, without special reference to the meeting of the British Medical Association in Dublin last August. It is quite unnecessary for me to recall to your minds the splendid hospitality of Trinity College, of the honoured and distinguished President of the Association, who at that time so worthily presided over us in this College, or the other Societies who vied with each other only in showing honour to our guests. But I must say that if what I have pointed out be true, as to the value of a Society like this, embracing only one city, it must be more strikingly true of Societies embracing a province or a kingdom.

The pursuit of truth ought to be intensified, and the discovery of truth rendered infinitely more probable by the collision of so many men of different capacities, of varied training, and of diverse experience.

But, in such a gathering, there is more than this. "As iron sharpeneth iron, so the countenance of a man his friend," and from the personal contact and intercourse of men of the same profession and the same pursuits, there is, if I may so say, a vital electricity elicited which radiates through the whole body. Men whom we have known by repute, by their writings, become much more to us when personally acquainted, and I am sure many felt, as I frankly confess I did myself, stimulated and encouraged to work more diligently, and to think more earnestly, after that meeting than before.

Still a further good remains. We are not merely scientific inquirers nor social individuals, but members of a corporate profession, and if it have been felt hitherto that a want of union has been a crying evil in our profession, surely the blending of the entire profession into one body bids fair to correct this evil, and to give to us the advantages we need in our corporate capacity.

For these reasons, gentlemen, I feel bound to regard the meeting of last August, the first of the kind in the history of our profession, as the most important event that has occurred in my time, and opening a new era of improvement and advantage for us.

Gentlemen, it is as true of each generation of a profession as it is of an individual, that "no man liveth to himself," but for good or for evil we influence others, and are influenced both by those who have preceded us and by our contemporaries. "One soweth and another reapeth," is the old law of all progress, moral, social, or professional.

But if we gather up the rich stores handed down by our predecessors, if thus we are "heirs of the ages" past, let us not forget that we are trustees for the future. We are responsible for the transmission of our inheritance, not as we received it, but cultivated, improved, and enlarged.

Now, gentlemen, if I may trespass a little longer on your patience, I will pass from these generalities and direct your attention for a short time to some of the pathological reflex irritations we meet with in women.

I have selected this subject because of its great practical importance; not that these irritations involve danger to life, but they are so frequent that the practice of gynecologists consists very largely of them, and some of them simulate organic affections so closely that it requires some care and experience to avoid mistakes.

The frequency of these reflex disorders in females is remarkable; so far as I have any means of judging, they are much more common than similar disorders in males, partly, perhaps, because of the greater sensitiveness of the nervous system in females, and partly because there are no organs in the male which occupy the same influential position as the uterus and ovaries in females. Unless we realize this we shall be very much at sea in our management of such complaints.

We are consulted about these affections during the entire active period of uterine life; they are not very frequent until some years after puberty, but after marriage, even at an early age, they are sure to appear.

But the most peculiar circumstance about some of them is their occurrence, in a marked manner, about the time of the cessation of menstruation, and for some time afterwards, when the ovario-uterine functions are entirely in abeyance.

The principal reflex irritations which have come under my observation are the following:—1. Irritability of the bladder. 2. Pruritus of the external parts. 3. Ovarian pain. 4. Sciatica. 5. Nausea and vomiting.

About the "time of life" we may find any of the above, but more generally—1. Excessive secretion of air in the stomach. 2. Flushing and sweating of the face and head. 3. Pain about the umbilicus. 4. Headache and fulness.

I shall say a few words upon the most important of these affections.

1. *Irritability of Bladder.*—There are few uterine diseases which do not give rise to more or less of this troublesome complaint, but in some cases it becomes a fearful torment, especially at night. I have known patients obliged to rise eighteen or twenty times during the night to empty the bladder. In some cases, too, there is a degree of tenesmus which amounts to actual suffering. Only in a few cases have I found it to assume the form of incontinence of urine.

Generally speaking, the appearance and composition of the urine is natural; but in some cases, where the disease was of long standing, I have seen it loaded with lithates and mucus. Of course, the first question is as to the presence of a calculus, and, luckily, catheterism will generally solve this satisfactorily; and the next, as to what it is to be attributed. In the majority of cases it is the result either of granular inflammation of the cervical canal, excoriation of the cervix, or vaginitis; and if, there being no stone, we find either of these, we may be satisfied that it is the origin of the reflex irritation.

2. *Pruritus of the external parts* may exist without eruption or any apparent change in the skin; and in such cases no external application is of any use. Unfortunately these cases are generally of old standing, from the natural reluctance of women to consult any one about such a complaint. I have recently had under my care two ladies in whom it had existed fifteen or sixteen years, and one of them assured me that she had not had a whole night's sleep during that time. Like the irritability of the bladder, it is always worst at night. We see it at all ages. I have a patient of sixteen under my care at present, and more than one over fifty years.

I have no words to tell you the distress it occasions; it is far worse than pain. One patient told me that she was nearly out of her mind, and another that all her labours put together would be less intolerable.

Now, I believe in all cases where there is no eruption externally, you may regard the pruritus as a reflex irritation from either, 1. Vaginitis; or, 2. Excoriation of the cervix.

3. *Ovarian pain.*—Reflex ovarian pain from some uterine or vaginal disease is perhaps the most common of these irritations; but less complaint is made of it, because it is generally transient, though recurring. It may occupy the situation of either ovary, or both, but I should say that the left is more frequently affected. If you examine very carefully, externally and internally, you will find no tumour or enlargement, and no tender spot. The patient can breathe fully, and the pain is rarely increased by movement. So far, all the evidence you can obtain negatives the supposition of organic disease; and when, in addition, you find some disease of the uterus, I think you need have no doubt of the reflex character of the complaint.

4. *Sciatica.*—In these cases the pain resembles ordinary sciatica exactly in its seat and character, and in several cases I have seen the patient treated for sciatica.

I remember a very remarkable case which I saw through the kindness of Dr. Stokes. The lady had been treated in the country for sciatica, without benefit, and Dr. Stokes at once suspected some uterine ailment. When I saw her she could not stand upright, and was barely able to limp across the room. The pain was severe, nearly constant, and increased by movement. Menstruation was regular, but she had a leucorrheal discharge. On examination I found an irritable erosion not larger than a sixpence, and until I had tried the effect of curing this, I could scarcely believe that all the suffering was a reflex irritation. As the erosion got better the sciatica diminished, and by the time it was cured the sciatica had disappeared.

Another case I saw very lately was sent to me by a physician in London. She was thought to be suffering from ordinary sciatica, and had been blistered and cauterized, and otherwise treated without relief. My friend, who is an accoucheur, proposed an examination, and found a cancerous uterus. When I saw her by far the more severe part of her sufferings was the pain along the sciatic nerve.

Nothing in the locality or character of the pain will enable us to distinguish between true and reflex sciatica. Only by a careful internal examination can we arrive at a satisfactory conclusion.

5. *Nausea and Vomiting.*—In pregnancy this symptom is, of

course, a healthy reflex action, though even then it may be carried to excess. But if, as I think, the origin of it then is some irritation, occasioned by the presence of the child in the lower segment of the uterus, we should be prepared to find a similar effect produced by other irritations in this situation. And this is exactly what we do find. With many forms of uterine disease there is more or less sickness of stomach; in some cases I have seen it assume the periodic character and limitation of morning sickness. In a case under my care at present, where the lady has vomited more or less of every meal for some months, it has a uterine origin. I suspect the same in all cases of hysteric vomiting which have been recorded.

The only guide to a correct diagnosis is first to ascertain, by a careful investigation, the absence of organic disease of the stomach, and then to observe the effect of remedies applied to whatever form of uterine disease we discover.

I need not enter into details upon the reflex irritations developed about the "time of life," as it is called; the flushing and flatulency, the headache, and the pain about the umbilicus, as these are, I am sure, sufficiently familiar to all. I will merely make two remarks. 1. That, despite the fears of our patients, I have never seen any ill termination of these cases, nor do I believe that this period of life is, *per se*, more dangerous than another. More men die between forty-five and fifty years than women. 2. I believe that in all cases where these symptoms are unusually severe, something more than the physiological changes will be discovered by an examination. I have generally found either erosion or granular inflammation of the cervical canal or vaginitis. I would therefore strongly advise that an internal examination should be made in all such severe cases.

As to the diseases giving rise to these reflex irritations, they are easily enumerated, and I am inclined to think that these reflex effects are not in proportion to their intensity.

Vaginitis; vascular tumour at the orifice of the urethra or uterus; erosion of the cervix; ulceration of the cervix, whether simple or malignant; or granular endometritis, constitute, I think, the entire list; nor can I say that there is an invariable relation between the kind of disease and the reflex irritation excited. So far as my experience goes, I should say that any of the diseases I have enumerated may give rise to any of the reflex irritations, the only exception being cancer, which certainly produces the sciatic pain oftener than any other reflex irritation.

I shall not detain you further, gentlemen, with the details of treatment. The essential matter is to cure the original disease, and if you do so you will find the secondary affection disappear, *pari passu*, in a most satisfactory manner. I do not generally give internal medicine, except to keep the bowels free, with a tonic, if necessary, and perhaps an anodyne suppository.

ART. IV.—*Case of Empyema with Inequality of the Radial Pulses, and Paralysis of the Diaphragm.* By JAMES CUMING, M.A., M.D.; L.K.Q.C.P.; Professor of Practice of Medicine, Queen's College, Belfast; Physician to the Belfast General Hospital, &c.

THE following case[a] is, I think, of sufficient interest, in a diagnostic point of view, to make it desirable that it should be recorded.

Neal Kennedy, aged thirty-five, unmarried, labourer, was admitted into the General Hospital July 30th, 1867.

History.—Several members of his family died of tubercular diseases. Seven years ago he had been a patient in this hospital, the only record of his case to be found being the name of the disease for which he was admitted—"Hydro-pneumothorax"—and the period during which he was an inmate—namely, fourteen days. From this illness he states that he completely recovered. Two years ago he passed through an attack of typhus fever, for which he was treated in the Union Hospital of this town. With these exceptions his health has been always good; habits regular and temperate. He is a well-built man, pale, slightly bald, with an intelligent countenance, and rather prominent eyes.

About six months ago he began to suffer from pain in the left side, which he describes as having been of a catching character. He continued to be able to work for some months after the commencement of the pain. His strength and appetite, however, gradually failed, and his breathing became oppressed. He placed himself under medical treatment, and was blistered and purged without any benefit.

[a] Reported by Mr. Robert S. Hudson, clinical clerk.

At present he complains principally of weakness; he has some diarrhea; no pain in side; very little cough, and the dyspnea is not at all considerable. His appetite is tolerably good; decubitus on back or left side; he sleeps pretty well.

Physical examination.—The left side of the thorax is observed to be dilated, its intercostal depressions effaced anteriorly, and no respiratory movements visible. A soft, painless, inelastic tumour is observed external to the left nipple; its diameter at the base is about two inches, and it covers portions of the seventh, eighth, and ninth ribs; the skin covering the tumour is not discoloured, nor is there any pulsation in it.

The impulse of the heart is visible on the right side, below, and rather external to the nipple. On palpation no vocal fremitus is to be felt in the left side, except close to the spine posteriorly. The circumference of the left side, on the level of a line passing from the spinous process of the seventh dorsal vertebra over the nipple, is 21½ inches; that of the right 19 inches.

On percussion absolute dulness is found to extend over the entire left side, and for about an inch beyond right margin of sternum. No respiratory sounds are audible, except in the neighbourhood of the spine; no vocal resonance. Respiration puerile on right side. There is distinct bulging in the epigastric and hypochondriac regions, owing evidently to displacement downwards of the diaphragm and of some of the abdominal viscera. Some tenderness on pressure is complained of in the left hypochondrium, where the depressed and resistant diaphragm can be readily recognized by the finger.

Pulse 72; in the right wrist tolerably full and soft, in the left very small—a fact to which my attention was first drawn by one of the students who surrounded the bed. A similar difference, but not so marked, was observed in the carotids. No murmur with the action of the heart; sounds normal. Pupils equal.

After consultation it was determined that the thorax should be tapped, as there could be no doubt of the existence of pus in the cavity of the pleura. Before the period arranged for the operation, however, he began to bring up by coughing considerable quantities of pus, and I hoped that in this way the entire contents of the pleural sac might be evacuated. During two days and nights the matter continued to be expectorated; the entire quantity got rid of in this way amounting to fifty-six fluid ounces. The impulse of the heart was now visible at a point higher than before by about an intercostal

space, and the circumference of the left side had diminished by three-quarters of an inch, being now $20\frac{3}{4}$ inches. The amount of the expectoration was, however, diminishing a good deal, and the constant efforts to bring it up had become very distressing to the patient. He was totally unable, from this cause, to lie down or get any sleep. The pulse rose to 116, the difference in the two sides being still considerable; but it was thought rather less marked than it had been previously, and an inflammatory flush had appeared in the left hypochondrium, where tenderness had been previously noticed on pressure, pointing to the probable danger of perforation of the diaphragm in that situation. Under these circumstances I thought it would be unwise to delay any longer the puncture of the thorax.

The operation was performed by my friend and colleague, Dr. William MacCormac, the opening being made through the tumour and in the eighth intercostal space. At the time of the operation, and during the two subsequent days, 173 fluid ounces of good pus, devoid of odour, flowed from the opening. The operation was followed by considerable relief of the general symptoms; the patient was able to lie down and get some sleep. The intercostal depressions became now manifest; the heart passed over to the left of the sternum; the left radial and carotid pulses acquired fulness, and by the end of the third day no difference could be any longer observed in them. Indeed, more than one of the various medical men who had kindly examined the patient with me at different times, before the paracentesis, and while the inequality existed, on being asked to compare the pulses in the two wrists, were inclined to consider that the left was now rather the fuller and stronger. The discharge gradually diminished to about two ounces daily; the appetite and strength improved rapidly.

Without entering into details of the progressive changes which ensued, I may now give an account of the physical examination of the chest a fortnight after the chest had been tapped. The girth of both sides had diminished—that of the right side being now $18\frac{1}{4}$ inches, that of the left $18\frac{5}{8}$ inches. Taking the level of a line passing from the spinous process of the eighth dorsal vertebra through a point an inch below the nipple, we find that above this line the left side is resonant on percussion—duller than the right—but still moderately resonant. Within the same space vocal resonance and fremitus are distinctly recognizable, and the sounds of respiration are audible, being somewhat weaker and more harsh

than those of the opposite side. There is no trace of râle of any kind, nor is there any friction sound.

Below the line mentioned vocal resonance and fremitus are altogether absent; no respiratory sound whatever is audible. On percussion we find amphoric resonance in front when the patient lies on his back, and some dulness at the most dependent part posteriorly. The situation of the resonance and dulness changes with any change in the position of the patient, but does not reach a higher level than that of the line above mentioned. Pus flows pretty freely from the wound when the patient leans well forward, and a probe introduced through the wound can be moved freely in a large cavity. It was now evident that the lung had expanded considerably, but not sufficiently to fill the entire side, and that it had become adherent to the thoracic parietes, leaving between its base and the superior surface of the diaphragm a loculus filled with air and matter. The heart's sounds were quite normal, and unaccompanied by any echo or splashing.

With the diminished circumference of the upper part of the thorax, the bulging at and below the base became more noticeable, and a curious phenomenon became observable—namely, a depression of the prominent portion of the epigastrium and hypochondria, during inspiration, and an elevation during expiration; both these movements being the opposite of those observed during health. This depression was more evident when a somewhat forcible inspiration was made, and was much more marked on the left side, but even on the right side it was distinctly observable both by inspection and palpation. The amount of discharge was now about one fluid ounce daily. There was scarcely any cough; respiration free; appetite good, and strength fair. The patient remained up the entire day, and walked a good deal about the ward in which he was. From this period until he left hospital there was little change in the symptoms. The bulging diminished gradually, but the abnormal condition of the respiratory movements continued, although no longer recognizable, except when the hand was placed on the surface of the epigastrium or hypochondria. He continued to improve steadily, with the exception of some inflammation of the lymphatics of the right leg, which yielded readily to rest, and the application of the flannel bandage recommended by Sir Dominic Corrigan. He was able to remain some hours daily out of doors, and left hospital at his own request at the end of September.

About the end of October I visited him, at his own request, at his mother's house, in a filthy little street in one of the oldest and most neglected parts of the town. I found that he had been for some time getting worse; he had not been able to get nutritive food, and he had ceased to take the cod-liver oil which had been prescribed for him. The discharge had increased and become very offensive in smell, and diarrhea had set in, with a good deal of irritability of stomach. There was scarcely any cough; no expectoration; and the condition of the chest was very little changed. I advised him to go to hospital again, which he wished to do. However, about a fortnight after I learned that he was dead.

I have not entered into any account of the treatment, as there was nothing worthy of remark in the measures employed. Tonics—a moderate amount of stimulants and cod-liver oil forming the staple of the treatment.

Remarks.—All observers agree as to the fact of the pulse remaining unaffected in cases of empyema. A case by Larrey, in which extreme feebleness of the pulse in the large arteries coincided with cardiac displacement, is quoted by Walshe,[a] who is not inclined to consider that there was any mutual dependence between the two circumstances. Murmurs with the sounds of the heart have been observed, probably dependent on torsion of the aorta, in cases of great cardiac displacement, and an instance[b] is given of empyema of the left side, in which an aortic bruit, the result no doubt of pressure, was audible from the last rib upwards along the left side of the spine for about five inches. I am not aware, however, of any case being on record in which a difference in the pulses was noticed. In a recent and excellent work[c] on diseases of the lungs, the equality of the radial pulses is referred to as a diagnostic mark which may be relied on in cases of doubt as excluding the idea of empyema. Had the empyema been a pulsating one, as in the well-known cases reported by Dr. M'Donnell,[d] the diagnosis might have become very embarrassing. It would be easy to suggest modes by which this phenomenon may be caused. My own opinion inclines towards seeking for the explanation in some narrowing of the arteries by torsion or otherwise, inasmuch as the absence of any evidence of venous

[a] Diseases of the Lungs, 3rd ed., 1860, p. 263.
[b] M'Donnell. Dublin Journal of Medical Science, Vol. xxvi., p. 437.
[c] Fuller. Diseases of the Lungs, 2nd ed., 1867, p. 184.
[d] Dublin Journal of Medical Science, Vol. xxv., p. 1.

congestion puts the idea of its having been caused by direct pressure of the fluid out of the question. Whatever be the explanation, however, the possibility of the existence of an inequality of the radial pulses in a case of empyema, the inequality disappearing after the evacuation of the fluid, is a fact of some clinical importance.

The alteration in shape and movements observed at the base of the thorax and at the upper part of the abdomen evidently depended on the condition of the diaphragm. The paralysed and probably degenerated condition of this muscle, caused by the long-continued pressure, and probably, also, by the effect of the inflamed state of the pleura, prevented the action which is normally exerted on the base of the thorax by the fibres which arise from the ensiform cartilage, and from the cartilages of the lower ribs—an action which is antagonistic to any forward movement of that region. Besides, the depression of the diaphragm, as well as of the upper abdominal viscera in contact with it, owing to the great mechanical pressure exerted by the superincumbent mass of fluid, would tend powerfully in the same direction. It was very remarkable, however, that the bulging should have continued after the removal of the great bulk of the fluid. This can only, I think, be accounted for by supposing that the diaphragm had contracted some adhesions with the parts with which it had been brought into contact during the continuance of the pressure. Duchenne,[a] criticising Dr. Stokes'[b] observations on the condition of the diaphragm in some cases of empyema, observes, with justice, that paralysis of this muscle must, of necessity, be followed by an increase of its natural convexity, and by its becoming more elevated in the cavity of the thorax, owing to the action of the abdominal muscles. Some cause must accordingly have been in operation to retain the muscle in its anomalous position.

The explanation of the reversal of the usual respiratory movements is also to be looked for in the passive condition of the diaphragm, allowing it to yield to the action of atmospheric pressure when a partial vacuum was formed in the chest by the action of the other respiratory muscles during inspiration, and the elevation during expiration to the pressure exerted by the lungs then acted on by the elasticity of the thoracic parietes.

[a] De l'Electrisation Localisée. 2nd ed., 1861, p. 727.

[b] Dublin Journal of Medical Science, Vol. viii., p. 197.

Duchenne[a] suggests that valuable indications may be drawn in cases of empyema from a deficiency of isochronism in the movements of the thoracic and abdominal walls during inspiration and expiration, and gives some cases of empyema observed by Racle and others, as well as by himself, in which the hands placed one on each hypochondrium were moved in contrary directions during respiration—that placed on the sound side being elevated during inspiration and depressed during expiration, while that placed on the side where copious effusion existed was moved in the contrary direction. When this phenomenon is observed, he thinks that the diagnosis of paralysis of one-half of the diaphragm may be made, and that a probability is established, as had, indeed. been suggested by Dr. Stokes, of the effusion being the result of an acute inflammatory process, and not a simple hydrothorax. In this case Duchenne's sign did not exist, the action of the diaphragm—although the empyema was single—being null on both sides. At the same time some weight must be attached to the great amount of malposition of the diaphragm, altering completely the direction of its muscular fibres.

ART. V. — *Observations on the Treatment of Stricture of the Urethra by the "Stricture Dilator."* By ROBERT M'DONNELL, M.D., F.R.S., Fellow of the Royal College of Surgeons; one of the Surgeons to Dr. Steevens' Hospital, Dublin.

MR. BERNARD HOLT, in his work on the immediate treatment of stricture of the urethra, very correctly observes that nature will not cure a chronic stricture. The question is, how far can we by art hope to effect a complete and permanent cure of the complaint? to what extent is this object attained by the so-called immediate plan of treatment?—are all cases equally suited for the employment of the stricture dilator, and if not, how are we to discriminate those which are most suitable for it?

These are questions which have still to be answered, and in adducing some cases which go to show that perfect recovery does not always follow this mode of treatment, I hope I shall not be understood in any way to detract from the merits of Mr. Bernard

[a] Loc. cit., p. 728, et seq.

Holt, who, by his resolution and perseverance, has now fairly established a mode of practice which many will admit to be among the greatest improvements which surgery has achieved of late years.

Every hospital surgeon knows how difficult it is to follow cases after they leave hospital; when we wish to obtain precise information on any point in which length of time is an element, we then learn how hard it oftentimes is to know the result of operative proceedings after the lapse of some months or some years. The cases which I have now to record were, however, easily kept in sight for some time. The patients were inmates of the Mountjoy Convict Prison, and I have been able to ascertain their exact condition after operation, partly from personal observation, partly through the assistance of Dr. Kelly, of Spike Island Prison, and Dr. Travers, of Rostellan.

In all the cases recorded in this communication the directions for the operation given by Mr. Holt were followed. In the instances in which the stricture was very close I dilated it with bougies for some days, until a No. 4 instrument could be passed. I always operated upon the patient standing, his back firmly fixed against a wall. The instrument is more readily introduced in this position than when the patient is lying down. I may add that I have never found it necessary to use a dilator with the directing-rod hollow; if the patient has for a time retained his urine when the dilator is fairly introduced into the bladder, I have invariably found the water to flow along between the blades of the instrument, not leaving any doubt of its being in the bladder. In fact, I should lay it down as a rule, never to be departed from, that unless the water flows in this way, so as to make it a matter of certainty that the bladder has been fairly reached, the dilatation should not be proceeded with.

Although in these pages I mention only those cases which I was able to follow carefully, owing to their being kept within prison walls for some years, yet I may say that besides these I have operated on a considerable number of others, both in hospital and private practice, yet I have never met with any untoward result; in two cases only did any considerable constitutional disturbance supervene, and this subsided after three or four days. When I compare this with the ill effects I have myself witnessed, as well in instances of internal as external section of strictures, and even following too hasty dilatation with bougies, it is, I must confess, perfectly astonishing. In short, were it not for the facts staring us

in the face, one could not believe that the forcible splitting of an old stricture could do so much good and so little harm. It proves how essentially tentative everything in surgery is.

When operating on my first cases I had the benefit of Mr. Rawdon Macnamara's assistance. To this gentleman we are indebted for the introduction of this operation into Ireland, and his earnest advocacy has not only won for it a high place in public favour, but has in a great degree overcome the timidity of those practitioners who, like myself, at first regarded with fear a procedure which seemed so heroic.

The following is a table of the cases operated by me upon in the Mountjoy Prison:—

TABLE OF CASES, ETC.

No. of the Case	Name	Age	Date of admission to Hospital for operation	Probable duration of the stricture	Date of discharge from Hospital	Subsequent Condition
1	M. L.	30	1 Aug., 1863,	about 3 yrs.,	21 Sept., 1863,	4 years after operation, no symptom of stricture.
2	G. W.	28	6 Aug., 1863,	4½ yrs.,	6 Aug., 1863,	4 years after operation, Dr. Travers reports slight stricture.
3	E. S.	37	21 May, 1864,	4 yrs.,	20 Aug., 1864,	3 years after operation, Dr. Kelly reports No. 9 catheter passes easily.
4	F. C.	27	28 Sept., 1864,	2 yrs.,	15 Oct., 1864,	1 year and 5 months after operation, no return of stricture.
5	R. H.	22	24 Sept., 1864,	2 yrs.,	15 Oct., 1864,	3 years after operation Dr. Travers reports—passes water in a good stream; No. 8 catheter passes.
6	J. R.	22	5 Dec., 1865,	1½ yrs.,	19 Dec., 1865,	1 year and 3 months after operation, no return.
7	J. M'C.	36	23 July, 1866,	6 yrs.,	3 Aug., 1866,	Relapse 4 months after operation.
8	J. M'C.	36	6 Nov., 1866,	—	7 Dec., 1866,	Died of cholera a month after second operation.
9	J. F.	24	1 Aug., 1866,	2½ yrs.,	15 Aug., 1865,	1 year after operation, no larger than No. 7 catheter can be passed.
10	P. K.	33	21 Sept., 1866,	3 yrs.,	8 Dec., 1866,	6 months after operation, No. 9 passes readily.
11	J. R.	33	21 Nov., 1866,	4 yrs,	8 Dec.,	6 months after operation, No. 9 passes readily.
12	J. C.	39	13 April, 1867,	4 yrs.,	10 May, 1867,	3 months after operation, No. 10 passes readily.

Now, on looking at this table the first question one is inclined to ask is—Is it certain that stricture really existed in each of these cases? Every surgeon who is candid enough to admit it will

confess that now and then patients have come before him complaining of small streams, cork-screwy, &c., and upon attempting to introduce a rather small, or even moderate sized instrument, he has found some obstruction, and concluded that there is a stricture; yet afterwards it may be with a larger instrument or a different curve he has easily got into the bladder—in fact, some lacuna or fold of the mucous membrane—some hitch in the vicinity of the triangular ligament; or it may be some enlargement of the prostate has offered a temporary obstruction to the instrument; yet no stricture has really existed.

In all of the foregoing cases I had recourse to an expedient which, I conceive, leaves no doubt that stricture actually did exist. I used a double-length catgut bougie, made with a gum elastic catheter to slide over it—an instrument devised, I believe, by the late Dr. Hutton, and much used by that eminent surgeon.

The catgut bougie (of small size) is first introduced into the bladder; being double the usual length, a long piece projects from the urethra; on this a catheter, open at each end, is slid; the end of the catgut being held, the catheter is slipt on, and is thus conducted along the urethra; if it comes to a dead stop at any point, it there has met with a stricture too close for it to pass. The narrowing permits the catgut to get through it; to the catheter it says no. According to the size of the catheter we know the size of the stricture. No lacuna, fold of membrane, false passage, or enlargement of the prostate can now deceive us. The conducting bougie would certainly steer the catheter past any such obstructions. I think this method may be considered a crucial test for the existence of stricture, and it was applied in each of the cases mentioned in the above table. I am then, I believe, justified in stating that stricture existed in every one of these patients, as in no one of them could I at first slip a No. 2 catheter along the conducting bougie into the bladder.

At the time (August, 1863) when I operated upon cases No. 1 and 2, Mr. Rawdon Macnamara and Dr. Cruise were present. They will remember, I do not doubt, that one of these patients made a considerable outcry, and that in his case the thrusting in of the dilator required an amount of force which to me, as a beginner, was alarming. The other patient (G. W.) merely said, "Oh, oh," and looked astonished at the unaccustomed sight of a large stream gushing from a large-sized silver catheter. Very little force was necessary, and the introduction of the dilator was accompanied by a

slight snap, as if a thread tied round the urethra had given way. This patient was surprised when he was directed to go to bed, as he said the operation had cost him no more pain than the introduction of an ordinary instrument. This case (No. 2, G. W.) is a most important one. Two months before this he had been in hospital, under treatment by gradual dilatation; six weeks after leaving hospital, with a urethra able to admit a No. 9 bougie, I could not pass No. 2 along the railroad catgut. Yet this stricture, which, under ordinary treatment, has such a tendency to return, is split, with little or no pain, and four years after he is reported as having but slight stricture.

Case No. 5 (P. H.)—Was examined by Dr Cruise with the endoscope some months after he had been operated upon; indeed this is one of the cases alluded to by Dr. Cruise in his classical paper on the Endoscope in No. 78 of this Journal. We perceived a transverse slit, somewhat crescentic in form, at the spot where the constriction had been.

Nos. 7 and 8 relate to the same individual. This is the only case in which anything of rapid return of the stricture took place, and as the patient died of cholera within a month after the second operation, and as an opportunity was afforded of examining the urethra, the case is one of importance.

I exhibited the bladder and urethra at the Pathological Society on February 16th, 1867.

The following is the report as published in the proceedings of the Society:—

"Dr. R. M'Donnell brought before the Society the bladder and urethra of a patient who had suffered from a stricture, which, shortly before the individual's death, had been treated by the method called 'bursting.' The following was the history of the case:—J. M'C., aged thirty-six years, was admitted to the hospital of the Mountjoy Convict Prison, on July 23, 1866. He stated that he had been suffering from stricture of the urethra for six years. He could only pass water in a very small stream, or in drops. At first only a small catgut bougie could be introduced; after dilatation up to No. 4 was accomplished Holt's dilatator was introduced, and the stricture was burst. Immediately after a No. 10 silver catheter was introduced into the bladder. All went on well, and four days later the urine was drawn off through a No. 10 silver instrument. He was discharged from hospital in August

3rd. On November 6th he was again admitted to hospital, stating that the stream had become as small as ever. On examination it appeared that it was not possible to introduce into the bladder an instrument larger than a No. 1 catheter. At his own request the operation of bursting was again performed as before. He was dismissed from hospital on December 7th. He died suddenly of cholera on December 22nd. Dr. M'Donnell removed the bladder and urethra, and they were examined carefully by Dr. Cruise, Mr. William Stokes, and himself, soon after removal. The appearance, however, at that time was not materially different from what the members had now an opportunity of seeing. A No. 9 catheter could readily be passed along the urethra. Except for the hypertrophied condition of the muscular coat of the bladder, and the dilatated state of the portion of the urethra behind where the stricture had been, there was no other sign of the disease having existed. There certainly did not appear to be any granular condition of the mucous membrane in the vicinity of the triangular ligament where the stricture had existed, neither was there any marked induration of the submucous tissue. Dr. M'Donnell had at first thought that there was an appearance indicating that the dense fibrous, submucous tissue had been split by the process of dilatation, while the mucous membrane itself had been only slightly, if at all, lacerated. On inspecting, however, other urethræ he found a similar appearance. The interest of this case was, perhaps, in the fact that it showed so little sign of disease remaining. The case was one in which the stricture had unquestionably returned in a comparatively short time after being burst."

Besides the cases given in the table, I met with two cases among the convicts in the Mountjoy Prison, in which the immediate plan failed to afford even temporary benefit. As they were cases of quite an exceptional character, I do not think it would be fair to group them with ordinary cases of stricture.

A military prisoner, committed for an offence of the most serious character, gave the following account of his case:—Some years before he had a bad stricture, which became complicated by perineal abscess. A surgeon with difficulty succeeded in getting a gum elastic catheter into his bladder, and having succeeded, he left it there for a month. When it came to be withdrawn it had to be dragged out with great force, and as the patient described it, it was encrusted with calculous matter, so that the end of it was as thick as his little finger. Of course the urethra was lacerated from

one end to the other, and when he came under my notice the urethra beneath the penis, as far back as the scrotum, was gone. I could, with difficulty, introduce a small silver probe into the orifice, from which the water came. Indeed I could not do so until the patient, holding up the penis, forced out some urine from the little orifice in the angle between the penis and scrotum.

The whole urethra had, in truth, been destroyed, and there only remained a fistulous trajet from the bladder, where the urethra formerly had been. I gradually dilated this channel with bougies, but it narrowed up again with extreme rapidity At last, in despair, I introduced the dilator, and burst it. It did no harm, but it did no good; the tendency to narrow was as great as ever. The patient was obliged to keep his disease at bay by the daily use of a bougie kept in for half an hour.

The second case to which I allude was a very similar one, in which almost the entire urethra had been made into a fistulous channel by a strong injection of nitrate of silver used to cure gonorrhea.

No portion of the urethra had ever entirely sloughed away, but abscess had formed, and fistula and the whole length of the urethra was of almost cartilaginous firmness. In this case there was great difficulty in dilating the urethra, and it narrowed again at once. I now also tried the dilator. Here also it did no harm, but it did not at all check the tendency to close again. After one day had elapsed it was impossible to introduce so large an instrument as on the day before.

I am clearly of opinion that is not, in general, good practice in the treatment of stricture to introduce very large instruments, as No. 12 or 13. I have satisfied myself by observations made by injecting the bladder and urethra with wax, that even in subjects of large size, the portion of the urethra begirt by the triangular ligament rarely exceeds the dimensions of a No. 10 catheter. To dilate, therefore, beyond No. 10 or 11 is, then, to dilate beyond the normal calibre of the canal; the urethra is kept in a state of irritation by this, and generally resents such treatment. I am convinced by experience that when the regular introduction of an instrument is necessary in order to keep a stricture at bay, it is, as a rule, better not to go beyond what may be considered the normal size of the canal at the triangular ligament, and, as I have said, this does not generally exceed No. 10.

In three cases only of those which I have tabulated was there any

decided tendency in the disease to return observed, and in only one to return promptly. To what was this tendency to return so quickly due? This is a question which I am at a loss to answer. I must content myself by recording the fact. Dr. Cruise, whose frequent use of the endoscope has made him familiar with the pathological changes in the urethra, is inclined to believe that the immediate plan will be found best suited for cases of true organic stricture, and that the obstruction arising from chronic inflammatory constriction, produced by the granular state of the canal, is less likely to be benefited by it. Yet we could find no evidence by the careful examination of the urethra in this case, that granulations had existed; but then it must be recollected that the stricture had been treated a second time, and, for so far, with success, at the time when the opportunity of examining it had occurred.

ART. VI.—*Reports in Operative Surgery, from the County Down Infirmary.* By JOHN K. MACONCHY, M.B.; F.R.C.S.I.; Surgeon to the County Down Infirmary.

I. CASE OF AXILLARY ANEURISM AND AORTIC PATENCY—LIGATURE OF THE SUBCLAVIAN ARTERY—CURE OF ANEURISM.

II. CASE OF POPLITEAL ANEURISM—LIGATURE OF THE FEMORAL ARTERY—CURE OF ANEURISM.

III. POPLITEAL ANEURISM TREATED BY PRESSURE—OCCURRENCE OF ANEURISM OF AORTA DURING TREATMENT.

IV. AN OPERATION TO REMEDY THE DEFORMITY OCCASIONED BY THE CONTRACTION OF THE CICATRIX OF A BURN.

HUGH GRANT, aged seventeen years, farm labourer, admitted to the County Down Infirmary, 2nd Oct., 1867. A small, delicate-looking lad; below the average in muscular development, with an anxious, worn expression, having a pulsating tumour in the right axilla, about the size of a small hen egg, with the larger end above, on a level with the coracoid process, and the other end a little below the inferior edge of the great pectoral muscle; the whole arm is swollen, and of a colour approaching to livid; he can move the upper arm, but with difficulty; the sensation of the lower part of it is impaired; there is complete paralysis of both sensation, and motion, from the elbow down; the pulsation of the brachial and radial arteries is very weak, while on the opposite side it is jerking

and obvious. The pulsation in the tumour is unmistakably eccentric; there is a loud double bruit in it, and it can be easily emptied, when pressure is made on the subclavian artery; the coats feel thin; he complains of severe pain in the tumour and arm.

There is most remarkable visible pulsation in all the large arteries of the body; a loud double bruit de souflet is heard, with the heart's action, louder in the vicinity of the aortic opening than elsewhere, completely masking, or replacing, the second sound of the heart; it is propagated along the arteries with a gradually diminishing intensity.

The pulse is sharp and jerking, over 120, and seldom counting the same for two consecutive half minutes.

The dulness on percussion in the cardiac region is little, if at all, in excess of the normal amount. He has a troublesome hacking cough, not attended with any expectoration.

History.—He says that previous to four weeks ago he was quite healthy; could run, and jump, and was not shorter in the wind than other lads of his age. About that time he was engaged in mowing (the scythe is used more commonly in this country than the reaping hook for harvesting), when suddenly he was attacked with pain in the arm, soon followed by swelling, discoloration, and loss of feeling and motion, in the forearm. About nine days previous to admission here, he first observed the tumour in his armpit. He believes it to have doubled in size since then. His state on admission was such that I feared the cardiac and general derangements would bring about a fatal result before danger became imminent from the aneurism, so that at first, treatment was directed to these symptoms, and he was given a good nutritious dietary to get up his strength. After six weeks his cough was gone; the heart's action certainly more equable, though the pulse never fell below 120. His strength was improved notwithstanding frequent epistaxsis. During this time the aneurism increased very little, being now about as large as a good-sized hen egg.

Nov. 12th.—The brachial artery was found to have ceased pulsating altogether. This must have happened during the few previous days, probably from the slipping of a clot in an examination of the tumour. The pulsation, however, was as brisk as ever in the aneurism. Nature had, in fact, anticipated me in stopping the current on the distal side of the tumour (as I had intended to try distal pressure), with no good effect. Two days after a large vessel was felt in the musculo spiral groove; the pulsation was

unchanged in the tumour, and it could be emptied almost as completely as before.

After this the tumour increased more rapidly, chiefly by extension downwards, though it also approached the clavicle, and was pushing the bone upwards. During the first week of December it became evident that longer delay was inadmissible, as the rapid increase of size, and the thin feel of the coverings, threatened to make it a diffuse aneurism. Having ascertained that the current could be stopped in the subclavian for two minutes without any increase of cardiac irritability, I determined to ligature the third stage of the subclavian artery.

Dec. 7th.—Assisted by Dr. W. MacCormac of Belfast and Dr. E. Nelson, I proceeded to operate. The patient was placed on a table with the shoulders elevated and well situated for a nearly horizontal light. Chloroform was given by Dr. Nelson, the pulse being well watched. It, however, improved in steadiness and volume, as anesthesia increased. As soon as insensibility was complete, the head being well held over to the left by Dr. MacCormac, and the arm depressed as far as practicable by an assistant, the usual operation was performed, the steps of which it is needless to describe here with accuracy. Suffice it to say, I adopted the $\geq$ shaped incision, and it gave me little room enough. There were two external jugular veins, both of which I contrived to work over to the inner side of the wound. Except for the skin and superficial fascia, and for the fascia that binds down the homo hyoid muscle, the knife was not even in my hand. Under the deep fascia there was a regular plexus of veins, through which I pushed aside the areolar tissue with a director and forceps. Owing to the elevation of the clavicle the depth at which the artery was found was very great. The needle was passed from below upwards, and outwards. The clearing of it between the artery and the large root of the brachial plexus was attended with some difficulty; and when the artery lay in the bend of the needle it could still be felt pulsating.

A small artery was divided early in the operation, but Dr. MacCormac twisted it before it had time to obscure the wound. The sparing use of the knife saved the veins, of which there was an abundance, so that not half an ounce of blood was lost from first to last.

On tightening the ligature, pulsation was at once arrested in the tumour. The wound was washed out with a weak solution of

carbolic acid. The flap retained in position by two points of suture, and covered with dry lint and adhesive plaster.

Emesis occurred during the patient's recovery from chloroform. He was, however, placed in bed with a steadier, fuller pulse than when he was expecting the operation. The limb was enveloped in moderately warm flannel. He was completely free from shock, so that neither sedative, nor stimulant, was administered. Ordered to have nothing but a drink if he wished for it till seen again. Three hours after the operation, his only complaint was that the other patients had got their dinner and he had got none.

His cough returned in the evening, and annoyed him that night and the next day Ordered a mixture containing acid hydrocyan. sol. morphiæ, and ether chlor. On the third day the cough had disappeared.

Dec. 20th.—The fourteenth day after the operation he had an attack of epistaxis which lasted for some hours, but subsided under the use of acid sulph. aromat. and infusion of matico.

Dec. 21st.—Fifteenth day. The ligature came away with the dressings.

Dec. 31st.—Since the fourth day after the operation the pulse has been pretty constantly 104, except for part of the sixth day, when there was a little discharge from the wound, and for a few hours it was 120, but again returned to the 104, being at least 20 beats below its average before the operation. The tumour is now smaller than any hen egg, hard and pulseless. The arm is reduced to its normal size. There is a slight return of sensation in the forearm; when it is touched he feels a tingling in the fingers. As yet the muscles have not regained any power. However, bearing in mind how long paralysis occasioned by pressure on the axillary nerves in some dislocations of the shoulder lasts, I think there is every reason to hope that the improvement which has commenced will continue.

In these days, when so many ingenious devices are in the hands of the profession for the cure of aneurism by compression, it seems almost to require an apology for having ventured to resort to deligation; but in this case pressure would have been attended with great difficulties. If it had been attempted below the clavicle, it should have been sufficiently heavy to compress the artery through the upper portion of the great pectoral muscle; if above, it should have been sunk down forcibly behind the bone, so as to control the artery. In either case it was to me quite inconceivable that the

patient could have borne it without a prolonged state of partial anesthesia from chloroform or strong anodynes, either by the mouth or injected through the skin. Where such measures have to be resorted to, I feel convinced that if the operation of deligation is quietly, and cautiously, performed, avoiding the more superficial vessels so as to reduce hemorrhage to a minimum, and leaving as small a wound as is practicable, the patient suffers less pain, and is in less danger of constitutional disturbance, than he would be from compression with such adjuncts.

Some time ago, in a case apparently much more suitable for pressure than Grant's, I had, at the patient's request, to resort to ligature.

Thos. Andrews, aged twenty-seven, weaver, admitted to the County Down Infirmary 1st August, 1863, with popliteal aneurism, pressure on the femoral artery was tried both with the clamp and with the elastic instrument devised by Mr. Carte; but the patient being of an irritable temperament, would not suffer the compression to be sustained long enough to procure any appreciable change in the tumour. After several days of futile attempts to induce the patient to submit I determined to lose no more time, as the aneurism was large and becoming more and more superficial, the patient himself requesting me to do anything rather than persist in the pressure. So, on August 10th, I tied the femoral in the lower part of Scarpa's angle, there being nothing remarkable in the operation, except that I never saw the femoral vein. The ligature separated on the 21st day. The tumour was long in absorbing, and the stiffness of the knee remained till absorption was nearly complete. He left the infirmary quite well on the 21st October, 1863. I have frequently heard of him since as continuing well and able to work. He has, however, I understand, given up weaving.

That a sudden obstacle to the circulation in any artery may occasion mischief higher up, either in itself or the larger vessel from which it is derived, may be equally the case whether ligature or compression form the obstacle. That the latter may do so appears at least probable from the following case:—

Arthur Carr, aged fifteen, admitted to the County Down Infirmary 24th October, 1866, with popliteal aneurism. The lad, who was very intelligent, gave every assistance in his power, and pressure, with Signorini's instrument, under the constant superintendence of Dr. Edwin Nelson, was attended with the best results as to

the aneurism, which became solid, and pulsation in it was perfectly arrested; but just as we thought we had a most successful case the boy complained of pain in the abdomen and loins. In a very few days we were able to diagnose aneurism near the bifurcation of the aorta. That it arose after the pressure was commenced our frequent and careful examination of all the leading arteries left no doubt on our minds; nor, indeed, can it be looked on as an improbable result of such a complete check to the current of blood in a large artery. I only instance the case as showing that, in this particular, compression has no advantage over ligature.

In these remarks I do not wish to be understood as always preferring ligature. On the contrary, where applicable, I think compression ought always get a fair trial. But recently the medical periodicals have teemed with devices simple and complicated, whose chief aim seems to be to put purse silk out of use in surgery, which I think bad treatment for so trustworthy and reliable a servant.

To return to the case of Grant. The rapid progress of the cardiac affection renders it, I fear, improbable that he will live long, though his condition is in no way aggravated by the operation. The case is, however, an instructive one, both as to the distal occlusion of the artery failing to cure the aneurism, and that so large an artery at no great distance from the centre of circulation can be ligatured even with patent aortic valves, and no bad results ensue; it also adds one to the many cases where chloroform has acted well and safely in patients suffering from heart disease.

Edward Rogan, aged nineteen, a strong, healthy young man, admitted to the County Down Infirmary for the consequences of a burn received when he was three years old.

The cicatrix involves the lower part of the face, the front of the neck, and the upper part of the chest. The portion of the neck included between lines drawn from the angle of the jaw to the centre of the clavicle on each side, is in ridges and furrows occasioned by strong bands which have pulled the lower maxilla towards the sternum so forcibly that the mouth cannot be shut, and the position of the bone is so changed that the lower incisor teeth project almost directly forwards.

The lower lip is everted, so that the surface, usually in contact with the teeth and gums, presents in front; what had been the middle part of the free edge is on a level with the chin, and

adherent to it; the gums and teeth are bare, and the saliva dribbles over the everted lip.

The strain on the skin of the middle region of the face is such that it is with difficulty he can shut his eyes; the outer canthus of the lids is drawn downwards, and the aperture is oblique instead of transverse; there is a semicircle of cicatrix occupying the space below the internal halves of both clavicles and the upper sternal region.

The general health and constitution of the patient are good. He is very anxious that an attempt should be made to remedy his deformity. On careful consideration of the case I came to the conclusion that there was reasonable hope of improvement from operative interference, chiefly from the circumstance that the contraction did not depend on the superficial layer of cicatrix which replaced the skin, but was entirely due to the deeper bands. In cases where the surface cicatrix is itself the cause of contraction, or is so intimately incorporated with the deeper contractile bands as to be with difficulty separated from them, a good result after operation would be much less likely than in a case like the present one, in which the surface layer of cicatrix is soft, pliable, and, to a limited extent, movable on the deeper structures.

Where the contraction depends altogether or in part on the superficies of a cicatrix, if it is removed, Nature is compelled to replace it in some way, and does so almost invariably with one equally disposed to contract; if it is left it is not likely to change its nature after operation, and continues to contract.

Whereas, when the contraction is due to the deeper cicatrical bands, if they are removed, nature is under no such necessity to replace them, as she is to provide a covering for vascular and sensitive structures.

Another circumstance which encouraged me to operate was, that though the middle of the lip was adherent to the chin, yet for about a quarter of its extent, at each commissure, there was original lip edge still capable of being restored to a normal position, so that by following as closely as the case would permit, the operation recommended by Mr. Teale, a new lip might be made which would enable the patient to close his mouth, and prevent dribbling.

On 22nd May, 1867, the patient was placed on a table with his shoulders elevated, and his head thrown back. Chloroform was administered, the action of which he for some time resisted. When anesthesia was produced the operation was commenced by a semi-

circular incision bordering the cicatrix on the chest; a second incision passed from the chin to the centre of the first, and the angular flaps were carefully reflected without injury to the surface. The cicatrical bands were now exposed; they were taken singly and dissected out as the result proved almost completely, some of them sunk to a considerable depth, particularly at the inner edge of the sterno-mastoid muscles, where time and caution were required in order to remove them without injury to the important structures in the immediate vicinity. Having accomplished this part of the operation without injuring any vessel of sufficient size to require ligature, the lip had next to be dealt with. The indications here were to get increased depth of lip, and, if possible, old lip edge to form the margin of the new. This was managed by separating the whole soft parts from the chin, then, supposing the lip divided into quarters, a vertical incision of a little over an inch long was made at each side between the external and the median quarters. From the lower end of this incision the knife was carried outwards and upwards through the whole thickness of the lip and cheek for about two and a half inches. The upper edge of the middle half of the lip was pared so as to leave a clean incised surface. The two external quarters were drawn inwards so as to ride on the corresponding median quarters, and firmly sutured to each other, and the parts beneath all bleeding vessels being secured by the sutures.

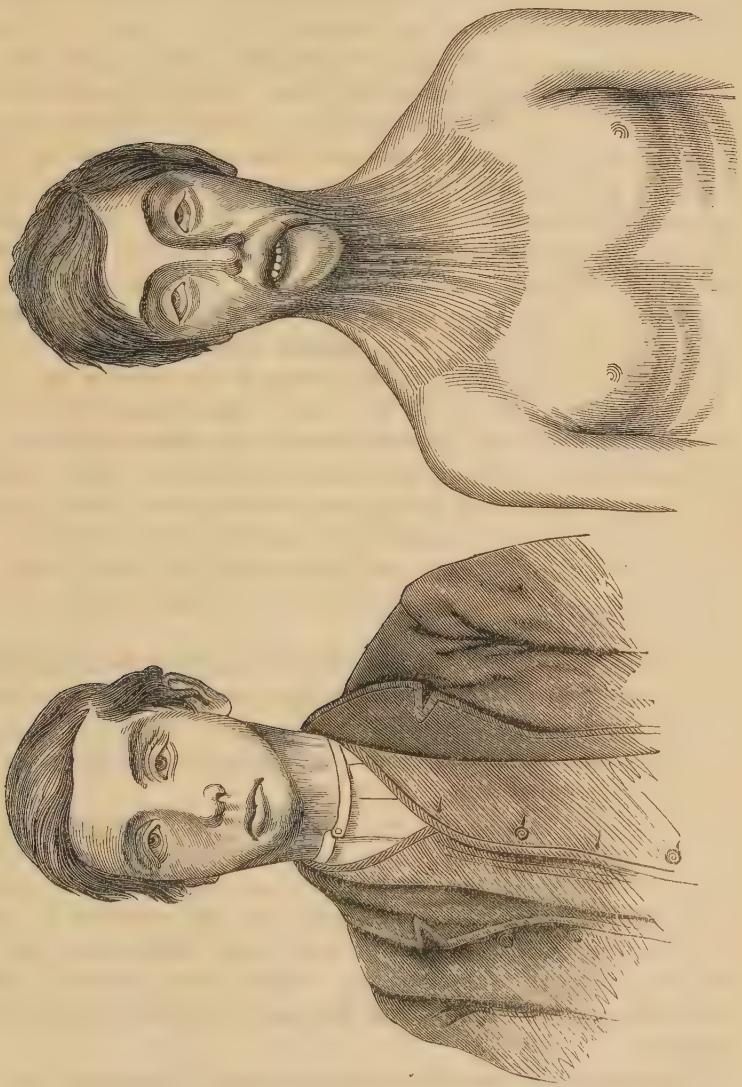
The head being still kept in the extended position the flaps were replaced, and covered the exposed surface exactly to the clavicles. They were retained in position by two points of suture; oiled lint was laid over the whole wound, and the patient placed in bed with his shoulders elevated and his head low.

The subsequent course of the case admits of being concisely sketched. After the operation there was considerable shock, requiring both stimulants and anodynes, and for some days reactionary fever had to be combated; yet the progress of the wound was favourable; the flaps speedily united to the parts underneath, the exposed surface on the chest became covered with granulations, and the component parts of the new lip united by adhesion.

The surface of the chest was slow in healing, but always had a healthy aspect, and during the whole time the extended position of the head was rigidly maintained.

Notes at short intervals of the case would be quite useless and

uninteresting, so I pass over an interval of three months to August 27th, when he left the infirmary, his state then being that he could move his head in any direction, could perfectly and easily close his



mouth, and in consequence the saliva no longer dribbled, the axis of the aperture of the eyelids is transverse, and he has a chin. In speaking of the removal of the cicatrical bands, I said they were

almost completely removed, because from an early stage after the operation there appeared to be the partial remains of one between the middle line and the edge of the left sterno-mastoid muscle; and now (Aug. 27th) there is still the remains of it there, though it is only observable in forced extension of the neck. It was, no doubt, a fault in the operation, but one not easy to avoid, as with a good deal of surface bleeding, if a band of the kind happened to get a nick or two and lose its prominence, it is hard to distinguish it from more harmless structures; in fact, the complete removal of the contractile tissue is the great difficulty of such a case.

From the 27th August till 6th January, 1868, he has daily worked at his trade of flax scutcher, and has now come to the infirmary at my request. Since the last note he has gained certainly in having a more accurate opposition of his incisor teeth. His lip is still perfect; it has not contracted in the least.

There is not a vestige of a contractile band, except the one before mentioned.

The whole right side of the neck is perfectly free and soft, and on the left side, external to the one band, it is equally so. This one band is made prominent by extension of the neck, but is so little inconvenience to him that he is far less concerned about it than I am. Notwithstanding this mishap, it will be seen on contrasting the two woodcuts (one taken from a drawing I got of his state before the operation, and the other copied from a photograph at the time of his leaving the infirmary, but which hardly does justice to his present state, as his mouth is now straighter), that he is amply compensated for his loss of time and the inconveniences of the after treatment, and that the success obtained fully warranted the operation.

ART. VII.—*On the Symptomatic Value of "Herpes Zoster."* By WILLIAM MOORE, M.D., Dub.; M.R.I.A., Fellow College Physicians, Physician to Mercer's Hospital; Physician in Ordinary to Sir P. Dun's Hospital, and to the "Institution for Diseases of Children;" Lecturer on Practice of Medicine; Examiner in Practice of Medicine, Coll. Physicians, &c.[a]

ON the night of the 7th November last I was called to see Mr. P——, aged sixty-four, who was suffering from violent pain in the left dorso-lumbar region, which radiated towards the umbilicus.

[a] Read before the Medical Society, College of Physicians.

He was writhing with the pain, still his pulse was little accelerated, and he had passed water freely. I desired hot fomentations to be applied over the seat of the pain, and gave him a full opiate.

Next morning (8th November) he was still complaining of a dull pain in the same region, but there was no discoloration of the part, or pain on pressure, and he had slept some hours. I prescribed aperient medicine, which relieved the bowels, and he passed water in the usual quantity.

At 11 p.m. I called again to see him; he was suffering great agony referable to the same region, the pain having returned with intensity almost exactly at the same hour as it occurred the night before. This periodic feature of it showed me its neuralgic character. Anodynes were applied, and an opiate given.

On the 9th the pain was less severe, and the patient had got some sleep. A line of erythema, not quite continuous, extended from above the umbilicus under the floating ribs on the left side, and in one or two places small vesicles were apparent.

I prescribed bark in effervescence, with bicarbonate of potash, liberal diet, and a full opiate to be taken at bed time, if the pain returned with severity. The pain did return about the same hour on the nights of the 9th and 10th. On the morning of the 11th the eruption had the following appearance:—About the mesian line there was a cluster of vesicles as large as beads, and full of a clear, limpid fluid, the cluster being surrounded by a red margin; another cluster, with vesicles of a smaller size, was situated about the margin of the floating ribs, while the erythema extended and ascended, in a partially broken chain, to the spinal column. On the 12th the patches of vesicles had formed towards the spine.

On the 13th the vesicles over the abdomen were becoming muddy, and of a darkish hue; in two days more some of them were broken, others livid and flattened, and at the end of ten days their outlines were comparatively obliterated, but the skin still remained tender, and in one or two places small cicatrices were apparent. Such was the course of this eruption, about eighteen days having passed from the date of the first neuralgic symptom till the eruption may be said to have disappeared.

The name "Herpes Zoster" may seem inappropriate for this eruption, inasmuch as it is non-parasitical, whilst the herpes we are most familiar with, the "circinatus," is of parasitic origin; but this is a point I will not stop to discuss. As to the recurrence of this affection it occasionally, though rarely, happens, and as to its seat

my experience leads me to think the right side is more prone to it than the left; but why the fourth and fifth dorsal nerves seem to be usually implicated, I am at a loss to explain, nor can I say what share, if any, the sympathetic nerve takes in the production of this eruption.

Dr. Von Barensprung[a] tells us that herpetic eruptions follow the course of various nerves; thus the "zoster facialis" corresponds to the cutaneous and mucous twigs of the trigeminal nerve, the labial form being restricted to the labial branch of this nerve. Then we have a herpes corresponding to the peripheric distribution of the third cervical nerve, also to that of the fourth; to the brachial plexus, and so on downwards.

Dr. Von Barensprung considers the source of this inflammation is not from without, nor in the blood; but that it operates through the nerves, and, in fact, depends upon their abnormal irritation. He considers that the posterior roots of the spinal nerves are those affected, inasmuch as this affection is associated with exalted sensibility; hence it is to the posterior roots and to the spinal ganglia which are connected with these roots, that we must look for an explanation of this phenomenon. Kölliker and others describe the sensitive nerve fibres as passing through spinal ganglia without being intimately connected with them, whilst the ganglionic fibres arising in the ganglion masses do not extend towards the spinal cord, but take a peripheric course along the sensitive fibres. Hence the neuralgia in zoster is caused by a propagation of irritation from the ganglion to the posterior roots, and thus the latter may propagate their irritated condition through the spinal cord to neighbouring and symmetrical nerve regions; whilst the trophic irritation always remains on one side, because the ganglion sends no fibres to the spinal cord, or receives any from it. He quotes one case in which intense pain at the posterior part of the whole leg, and the formation of numerous groups of vesicles with reddish yellow contents, of various sizes, was supposed to be owing to an œdematous and hyperemic state of the ischiatic nerve as found after death, connected with psoas abscess. He also quotes cases showing that a peripheric irritation of a nerve containing ganglion fibres may cause a limited eruption of herpetic vesicles. Dr. Handfield Jones,[b] in connexion with "eczema," believes that the actions of the affected part, of its tissue or its vaso-motor nerves, are

[a] Annalen der Charite-Krankenhausen zur Berlin, 1861.

[b] British Medical Journal, May, 1862.

deranged in some way, whilst Bernard tells us that paralysis of the vaso-motor nerves is capable of giving rise to inflammation.

Again, Dupuy observed that division of the sympathetic in the horse was followed by a peculiar dryness and adherence of the skin, with augmented perspiration and eruption.

In accordance with these views Dr. P. Gerhardt explains the presence of "herpes zoster" in more acute disease. He says the group of diseases, in the course of which "facial zoster" is frequently observed, is distinguished by initial rigors, or increased temperature; and that the eruption is caused by the fact, that the branches of the fifth nerve run through narrow, long canals, along with small arteries; these latter contract during the rigor, then dilate, and their abnormal size gives rise to pressure on the branches of the trigeminus and sympathetic; hence the "facial zoster."

Assuming, then, with von Barensprung that direct irritation of nerves is capable of producing severe inflammation in the parts to which they are distributed, which may even take the form of vesicles, I will give a brief outline of two cases.

Michael L—— was admitted into Mercer's Hospital, complaining of distress of breathing, difficulty in swallowing, severe pain radiating over the shoulders, and down the back. He had a hard, ringing, laryngeal cough, with aphonia. Both pupils were contracted, especially the right; there was dulness on percussion, and a second centre of pulsation over the top of the sternum; the respiration in the left lung was feeble; the left radial pulse could scarcely be felt; the veins over the top of the sternum were *too* prominent, and the patient had hemoptysis. In addition to these unequivocal symptoms of aneurism, or intra-thoracic tumour, he had latterly been harassed *with boils and herpetic eruptions* over the upper part of the chest, and between the scapulæ.

Case II.—Patrick ——, aged forty-four, was a patient in Mercer's Hospital. He suffered from neuralgic pains over the top of the chest and sternum; from a troublesome ringing cough; from dysphagia; from tightness of the skin of the left side of the face; from occasional intense heat of the left ear. There was slight drooping of the left angle of the mouth, and the left pupil was more contracted than the right. The superficial veins over the upper part of the chest were remarkable, and a tumour extended from the right clavicular articulation across the sternum. An *herpetic eruption* was present over the chin, neck, and chest.

Here were two cases of thoracic aneurism, in which we had unequivocal evidence of nervous pressure, and in which *herpetic eruptions* were present.

You may say the presence of this cutaneous eruption in these cases is a very trivial matter, and of little, if of any, practical value. I answer, fifteen or twenty years ago so would have been regarded contraction of the pupil, increased temperature with increased sensibility of half the face, or slight alteration in the muscles of expression, or increased flow of tears or modification of the voice, and yet what an advance these symptoms have given us in the diagnosis of a malady, till lately the most latent in the whole category of disease—namely, thoracic aneurism.

In cases where no positive local physical signs exist, the presence of any such symptoms as I have enumerated, coupled with dysphagia, dyspnea, inequality of respiration, hemoptysis, or any one or more of such phenomena, would point to pressure (especially nervous) within the thorax from tumour of some kind. Hence if such important strides have been made by these collateral nervous symptoms, and now if, in addition, we find that nervous pressure is capable of producing "herpes," we have another link in the diagnostic chain which we cannot afford to overlook.

ART. VIII.—*Fracture of the Thigh.* By LAWSON TAIT, Member of the Surgical Society of Ireland, &c.; Resident Surgeon, Clayton Hospital, Wakefield.

THE treatment of fracture of the femur in all its varieties has run with so many variations from one extreme to the other—from no treatment at all, to treatment most dangerously meddlesome, that it would serve no good purpose, were it even possible, to narrate a tithe of the wondrous mechanisms by which such an accident was intended to be remedied. Suffice it to say, that the two methods now generally in use are Desault's long splint, and the principle of the inclined plane, used either in the original plan of Sir Charles Bell, or by means of Liston's or M'Intyre's splints. The treatment by position brought forward by Pott is now, as far as I am aware, not in use; although it had, in suitable cases, several good points to recommend it. The double inclined plane seems also to have lost its hold on public estimation very much; except, perhaps, in cases of fracture immediately above the condyles. It is liable to the objections of causing œdema of the leg and foot,

that the position in which the limb is maintained is extremely irksome, and that it permits of but very slight change in position of the patient. In the use of the iron splints I have heard the patients complain bitterly of the cold. In children and old people it is scarcely possible to use this method.

Desault's long splint, although as perfect as such a simple apparatus could be, is far from being free from objections to its use. In the first place, children and old people are extremely intolerant of its use. It necessitates that the patient shall be continually on his back. When the fracture is near the middle of the thigh the proper arch of the femur is not retained; but, as Amesbury says, "Instead of forming a segment of a large circle it becomes two segments of two smaller circles," from the want of posterior support. There is very great difficulty in regulating the amount of extension by means of the perineal band. The theory is that there should exist no constant pressure on the perineum, but that the band should act as a check ligament. Practically, however, this is not the case, because to prevent shortening there must be constant action of the perineal band until union is fairly begun; and as few patients can stand pressure long it is not difficult to account for the fact that some surgeons declare that fracture of the thigh never gets well without some amount of shortening of the limb.

Under such circumstances it may be of some interest to bring briefly under notice the particulars of some cases in which I used a new plan of treatment of this fracture with most gratifying results.

The method referred to is that usually known as Buck's, and is described in Hamilton's elaborate treatise. Two years ago I had the good fortune to make the acquaintance of Dr. George K. Smith, of Brooklyn, U.S., who had been attached to one of the rear hospitals of the Federal army during the late war. Dr. Smith is the author of a most exhaustive paper on fractures of the neck of the femur, and on this account was allowed to select, as an especial study, cases of gunshot fracture of the thigh. His treatment consisted entirely in the use of Buck's method, and the success obtained by it has been extremely satisfactory. In the elaborate reports and catalogues issued by the Army Medical Department his cases are noticed; but I had the opportunity of examining photographs of many of his patients, and must express my opinion that the success obtained during that unfortunate war in the treatment of gunshot fractures of the thigh was such as immeasurably exceeded that of any other known campaign.

The principle on which the plan is based—that of continuous

extension by a weight and pulley—is not new, but the credit of adapting it successfully to the treatment of fractures of the thigh is certainly due to the American surgeons. The following will be found a convenient method of applying Buck's treatment:—Having placed the patient on a convenient bed, fasten two strips of ordinary diachylon plaster (that spread on unglazed calico is the best) reaching from about three inches above the knee to the malleolus, and from two to three inches wide, one on either side of the injured limb, and leave as much as will tie round a piece of wood adapted to the sole of the foot. This foot-piece must have a hole bored in it about three inches from the heel, through which a cord is run from above downwards and secured by a knot; it must also have a flat notch on either side in which the plaster straps are laid. The cord ought to be in front of the straps. The limb is now to be carefully bandaged from the toes to the upper ends of the plaster straps, and the bandage to be saturated with dextrin or paraffin. The foot of the bed is to be raised about four inches to obtain counter-extension by the weight of the body. The cord is to be reeved over a pulley, and have a weight attached to it. The fragments having been adjusted, four gouch splints are to be applied of suitable length on the four surfaces of the limb, and those are best retained in position by small straps buckled. A slight addition, which I have found of great importance in preserving the heel from injury, consists in having a small cradle placed over the foot, to which the foot is slung by a small cord running through a hole in the toe of the foot-piece; this also prevents the tendency to eversion which the natural form and axis of the limb create. The more robust the patient the greater the extending weight required, and more weight is necessary during the first week than afterwards. In children I have had good results with so slight a weight as four pounds; and in the case of a full-grown man I have had to use sixteen pounds. I have treated fractures of the thigh in all the various sites of fractures, and the result has been uniformly satisfactory. Of course, in cases of fracture close to or above the trochanter the splints on the thigh are unnecessary—the extension alone must be trusted to. It is not necessary that the splints should command the knee-joint. Indeed, except in cases when the fracture is very close to the condyles, I think it better that they should not do so. There is no object to be gained in restricting all motion of the hip-joint; indeed, all the cases have been allowed, if they wished it, to sit up in bed after the first week.

The true indication for treatment in all fractures is to reduce

the patient, as far as his injured part is concerned, to the condition of a crustacean, to supply him with a temporary dermoid skeleton until his vertebrate is repaired. The best illustration of the successful fulfilment of this indication is to be seen in the treatment of Colles' fracture by means of Professor Gordon's splint; and when we have equally efficacious means of treating every injury to the osseous system this department of surgery will be perfect.

The cases in which I have used this treatment are eleven in number—two compound and nine simple:—

Fracture	Sex	Age	Duration of treatment and result
COMPOUND			
Middle,	M	17	Closed wound with collodion; healed by first intention; up in seventh week; no shortening.
Middle, comminuted; removed a large loose fragment,	M	7	Closed wound with collodion; healed by first intention; up in ninth week; nearly a quarter of an inch shortening.
SIMPLE			
Upper third,	F	7	Up in fifth week; no shortening.
Middle,	M	8	Up in sixth week; injured limb distinctly longer than the other. He tumbled and repeated the fracture, and was again put up for five weeks; injured limb still longer than the other.
Two inches above the condyles,	M	70	Up in ninth week; no shortening.
Upper third,	M	33	Up in seventh week; no shortening.
Neck,	M	59	Up in tenth week; barely half an inch shortening.
Comminuted fracture of trochanters from direct violence,	M	61	Up in sixth week; no shortening.
Middle,	M	13	Firm union third week under treatment.
Three inches above the condyles,	M	27	Under treatment.
Lower third,	F	24	Up in seventh week; no shortening.

Besides the actually better results obtained from this method of treatment the patients are much more comfortable during its continuance than under any other that I have seen. In cases where bedsores threaten they can be more easily obviated in this method than in any other.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Fecundity, Fertility, Sterility, and Allied Topics. By J. Matthews Duncan, A.M., M.D., &c. Edinburgh: Black, 1866. 8vo, pp. 378.

In the volume before us Dr Matthews Duncan has examined, by the light of carefully and judiciously selected statistics, a great variety of interesting questions connected with the important function of reproduction. There are upwards of fifty different essays in the work, and as each of these is based on a series of statistical calculations, more or less extended, some idea may be formed of the amount of labour and research which this volume must have cost its author; but the best proof of this is to be found in the numerical tables, illustrating nearly every chapter, and of which there are no less than one hundred and thirteen, scattered throughout the different chapters. The sources whence he obtained the data necessary for compiling these tables have been various; but we should say that the Rotunda Lying-in Hospital, and the Returns of the Registrar-General for England and Scotland have supplied by far the greater portion of them. The former of these sources—by which we mean the clinical reports of Dr. Collins, of Drs. M'Clintock and Hardy, and of Drs. Johnston and Sinclair—has been a rich mine of information, which he has worked with great care and success.

We freely avow our conviction that it is hard to use statistics aright, so as to get from them "the truth, the whole truth, and nothing but the truth." To do this requires a clear, strong, and severely logical mind; but we unhesitatingly confess that the author of this work has given ample evidence of his possessing the above qualification in a very high degree. We doubt if any professed actuary could show greater discrimination and skill in dealing with the data here presented to us. Although this work is not what is called a "practical" one, still the information it brings out has a direct bearing on many practical questions of deep interest to the

obstetrician as well as to the political economist. We cannot go regularly through this volume in our analysis of its contents, nor indeed would this be necessary, as many of its essays have already been published; but we shall dip into it here and there, so as to show our readers the nature and results of some of Dr. Duncan's statistical investigations. The work is divided into ten "parts" or sections, each of which comprises several chapters. The enumeration of the titles or headings of each of the parts will give a very good general idea of the subject matter of the book:—Part I. On the Variations of Fecundity and Fertility of Women according to age. Part II. On the Weight and Length of the newly-born Child. Part III. On some Laws of the production of Twins. Part IV. On the Laws of the Fertility of Women. Part V. On some Laws of the Sterility of Women. Part VI. Note on Formulæ, representing the Fecundity and Fertility of Women (by Professor Tait). Part VII. On the Mortality of Childbed, as affected by the number of the Labour and the Age of the Mother. Part VIII. On the Age of Nubility. Part IX. The Doctrine of the duration of Labour. Part X. On the duration of Pregnancy.

In the course of the introductory observations to Part I., the author lays down a distinction between *fecundity* and *fertility*:—

> "By fertility or productiveness I mean the amount of births as distinguished from the capability to bear. This quality of fertility or productiveness is interesting chiefly to the statistician or the political economist. When a population is the subject of consideration it does not even involve the capability of every individual considered to bear, nor even the conditions necessary for conception. By fecundity I mean the demonstrated capability to bear children: it implies the conditions necessary for conception in the women of whom its variations are predicated. This quality of fecundity is interesting chiefly to the physiologist and physician. In short, fertility implies fecundity, and also introduces the idea of number of progeny, while fecundity simply indicates the quality, without any superadded notion of quantity" (p. 3).

If we have taken in the correct meaning of the author in this passage, we might say that fecundity has somewhat of a prospective reference, whilst fertility is retrospective—the one implies what the woman may do in the way of procreation, and the other what she has done. But in our estimation fertility is the only proof we can have of fecundity; hence we cannot see the use or advantage of laying down the above distinction in a statistical inquiry, nor how

it can be strictly adhered to. We may be rather obtuse on this point; but if so it is some consolation to find we are not singular, since Dr. Granville—one of the latest writers on this subject—has confounded the two. The age of a woman is always a delicate point, and hard to be ascertained correctly. Dr. Duncan seems fully aware of this, and frankly admits that it is "an unfortunate element of error, which creeps into the most carefully-prepared vital statistics." For our own part we never found a woman in her first pregnancy to admit she was over twenty-eight or twenty-nine! It is a current opinion that first children are, on the average, larger than subsequent births, and thence one cause for greater difficulty of first labours. But Dr. Duncan's tables do not confirm this opinion, but tend rather to show that the size of the fetus is more influenced by the age of the mother, and, in fact, follows the law of fecundity, being greatest when fecundity is greatest, viz., between twenty-five and thirty years, after which the fecundity of the mother and weight of the child undergo a gradual diminution. It is worth noticing with the author, that this result goes a certain way to account for the increased difficulty and danger so often experienced in first labours where the patient is no longer young; and the operation of the same law will also help to explain why it is that an old primiparous woman passes through labour more easily than one who is somewhat younger.

The statistics with regard to twin births bring out a very curious and unexpected result, namely, "that from the earliest childbearing period, till the age of forty is reached—that is, till a period when fecundity has become extraordinarily diminished—the fertility of mothers in twins gradually increases." Thus, of mothers from fifteen to nineteen years of age, only every 189th had twins, whilst those whose ages ranged from thirty-five to thirty-nine years (when the climax in fertility of twins was reached), every 45th woman produced two at a birth, or fully four times as many as the mothers under twenty. To this general statement there is one exception, in the case of first pregnancies, when there seems to be a somewhat greater chance of twins than in either second or third labours. As the age of mothers necessarily increases as the number of the pregnancy increases, it might be urged that the latter, and not the age of the woman, causes the increased frequency of twin births with advancing years. But this objection is satisfactorily met by Dr. Duncan's thirty-second table, from which it is evident that, speaking generally, twinning becomes rarer as the number of the pregnancy

increases. The author's tables and conclusions with regard to twin births do not contradict, but, on the contrary, rather support, the popular notion that twinners are, as a general rule, more fertile than uniparous women, so that the proportionate frequency of twin births may be taken as an index of the fertility of a population. If this be so it makes the fertility of Irish women appear to great advantage, as compared, at all events, with Scotch women. In the Rotunda Lying-in Hospital 16,385 women produced 480 twin children. In Edinburgh and Glasgow, in 1855, 16,301 wives produced 396 twin children; whereas they should, to equal the Irish women, have produced 477 twin children. Passing over many chapters which we have not space to notice, we come to that on "Expectation of Sterility," being the fifth chapter of Part V. His statistical tables in relation to this subject seem to warrant two general propositions, which are thus stated:—"*That the question of a woman's being probably sterile is decided in three years of married life.* For while a large number are fertile for a first time in each of the first three years of married life, only 7 per cent. of the fertile bear first children after three years of marriage, or about 1 in 13."

"*That when the expectation of fertility is greatest, the question of probable sterility is soonest decided, and vice versa.* For our tables show that of the wives married from twenty to twenty-four, who are all fertile, only 6·2 per cent. begin to breed after three years of marriage, while at the other ages, with less fecundity, a greater per centage commences after the completion of the third year of marriage."

The subject of sterility engages much attention at the present day, and many modes of cure (chiefly, however, of a more or less mechanical kind) are resorted to by the gynecologists of a particular school. Our author improves the occasion to advert to these, and does so in a way that leaves us in little doubt as to his estimation respecting them. "Besides being of evident intrinsic value, the conclusions here arrived at will afford to medical men means of estimating the utility of the many vaunted methods of curing sterility, which are now much in vogue, and which, considering the nature of the condition to be cured, justly excite anxiety for the honour of the profession in the minds of its best friends."

In Part VIII. the author enters upon a minute and impartial examination, by the aid of statistics, of two questions deeply interesting to obstetricians and to actuaries, especially at the present moment, when the causes of puerperal mortality are under such

close investigation. These questions relate to the influence exerted by the *number of the pregnancy*, and by *the age of the woman*, upon the mortality accompanying parturition. He candidly tells us he knows of no data sufficiently satisfactory to decide the questions raised, but so far as they go he uses the data available for the purpose, which are derived altogether from the clinical reports of the Dublin Lying-in Hospital, and of the registration returns of Edinburgh and Glasgow for the year 1855. Here, as elsewhere, it will suffice for us to reproduce the author's general conclusions, omitting the tables on which they are based:—

"1. The mortality of first labours is about twice the mortality of all subsequent labours taken together.

"2. The mortality from puerperal fever following first labours is about twice the mortality from puerperal fever following all subsequent labours taken together.

"3. As the number of a woman's labour increases above nine, the risk of death following labour increases with the number.

"4. As the number of a woman's labour increases above nine, the risk of death from puerperal fever following labour increases with the number.

"5. If a woman have a large family, she escapes extraordinary risk in surviving her first labour, to come again into extraordinary and increasing risk as she bears her ninth and subsequent children."

We have long been aware that primiparity had a considerable influence upon the mortality of childbed; but, in truth, till we read this chapter we had no conception of the magnitude of this influence. The above conclusions—or general laws as we may term them—cannot fail to suggest many reflections in the mind of every thoughtful physician. Dr. Duncan seems to think that the facts recorded about puerperal fever, viz., its occurrence specially among primiparæ and women who have borne large families—its pretty close correspondence in relative amount to the general mortality of parturition after different pregnancies—its subjection also to the law of the duration of labour—does not appear to him to lend support to the views hitherto generally entertained regarding it, and expressed in the words accidental, fever, contagions, epidemic. Another important reflection arises out of these laws, which is best given in the author's own words:—"Authors comparing the mortalities of lying-in institutions, whether from puerperal fever or from other causes, are frequently found neglecting to begin by ascertaining whether or not they are fit objects of comparison, and

under this head, *inter alia*, neglecting to ascertain the comparative amount of primiparity in each institution. It is plain that unless there be nearly the same comparative amount of primiparity in the institutions, their respective gross mortalities cannot be justly contrasted with one another."

These observations we consider to be of the utmost importance. We have before us at the present moment the report of a provincial lying-in hospital—very well managed, and apparently most successful in regard to the recoveries of the patients confined there; but there is one omission in the statistics—the number of primiparæ is not stated! This is a serious and vital defect, greatly lessening the value of the report itself, and leaving us in doubt how far the low rate of mortality is attributable to the hygienic arrangements of the institution. We know of one lying-in hospital in a large English town where, by the rules of the institution, women on their first children were excluded!

The age at which the mortality from parturition is at its minimum is about twenty-five years; and this deduction is arrived at after making due allowance for the influence of primiparity in swelling the mortality. In connexion with this law it is highly interesting to note that the same period of female life is that at which fecundity is greatest; and that during the whole of child-bearing life safety in parturition appears to be directly as fecundity, and *vice versa*. To have educed this wise and beautiful law—so entirely in keeping with the wisdom and goodness of the Creator as displayed in all His works—is surely a rich reward for the author's laborious researches on this subject, and justly entitles him to our warmest thanks.

What is the best age for a woman to enter wedlock? This is a question often discussed, and possessing deep universal interest. It will doubtless be admitted that the main objects of marriage, looking to the woman's side, are the bearing of children, the rearing of children, and the being a "help meet" for her husband. In view of these objects, then, let us see what information Matthews Duncan supplies to us.

In his chapter on "The Age of Nubility" we have a very full and satisfactory solution of the problem before us, in its threefold aspect. First, as to the bearing of children, he has demonstrated, in a previous part of the work, that the age at marriage found to be most secure of fecundity is the quinquenniad, twenty to twenty-four years inclusive. Next, as to the rearing of children, he adduces

data to show that there is a greater survival of children born of women married at twenty to twenty-five years of age than at any other.

Lastly, it is shown in other parts of his work that parturition at the period specified—twenty to twenty-four years of the woman's age—is attended with less danger to her health than at any other time. For all these reasons, then, we must consider twenty to twenty-five as the nubile age of women, in these countries at all events, whence the statistics are derived on which the conclusions rest.

We all remember the long and warm controversy that took place some years ago between Sir J. Y. Simpson and Dr. Collins, as to the effects resulting from mere protraction of parturition. Every man of much obstetric experience felt that in the main Collins had truth on his side; but there were few who detected where the fallacy lay under the formidable array of statistics which his skilful opponent brought to bear against him. It would be hard to say whether, in this great controversy, the application of statistics tended more to elucidate or confuse. "It is evident," says Dr. Duncan, "that accurate statistics can never yield false results, but false results are easily made to appear as if yielded by them. In other words, if a disputant resorts to statistics without the most careful use of logic, he easily flatters himself that they really supply the results he wishes from them." A memorable instance of this illogical use of statistics is supplied in the history of the subject before us. This great question, "The Doctrine of the duration of Labour," as it is termed, is one of immense extent and practical importance; and we feel bound to confess that in its investigation Dr. Duncan has displayed the most critical acumen, the soundest logic, and the most cautious induction. He has, with a master-hand, demonstrated incontrovertibly the great fact which Collins so zealously defended, that mere protraction of labour, *per se*, has but little influence on maternal mortality—that it is only an inconsiderable item among the many causes (single or combined) of the mortality of women in parturition and childbed. Our author fully concedes to Simpson—as all must do—the merit of formally enunciating the general proposition that the mortality increases with the length of labour. The pernicious tendencies of the doctrine, as laid down and applied by Simpson, are thus ably summed up:—

"Let us consider for a moment what such reasoning as Sir J. Y.

Simpson adopts in this controversy would lead to. It appears to us that, if he had looked whither his arguments might lead, he would have himself been probably deterred from urging them. If mere length of labour be an important element in the causation of deaths from labour, then certainly patience is no virtue in an accoucheur. If mere length of labour be as he describes it, then meddlesome midwifery must, I fear, be declared good instead of bad. If mere length of labour be as important as he represents it, then any treatment which will accelerate delivery may be easily defended. If it be right to disregard all the real causes of danger and death in labours, as this author does, in order to make prominent the danger of protraction, with the ulterior view of supporting an artificial interference which accelerates the process, then a like reasoning may be used to support the most absurd and unjustifiable measures, and the art of midwifery will be at the mercy of any specious reasoner, however ill-founded his arguments may be."

The few remaining chapters relate to the duration of pregnancy, and the mode of reckoning the period when labour may be expected. He very clearly points out the importance of distinguishing between *insemination* and *conception*, and adduces strong reasons for believing that in the human female, as well as in the lower animals, there is an interval between these two acts; but what the length of this interval is he does not attempt to define, it being, as mathematicians say, an unknown quantity. He is at issue with the late Professor Montgomery as to the duration of pregnancy, and shows, on Montgomery's own data, that the illustrious Harvey, in laying down 275 days as the normal period of human gestation, was more correct than the Professor in fixing 280 days.

The general conclusions at which Dr. Duncan arrives on the points in question are as follows:—

"1. That the interval between conception and parturition (the real duration of pregnancy) has not been exactly ascertained in any case.

"2. That the average interval between insemination and parturition (commonly called the duration of pregnancy) is 275 days.

"3. That the average interval between the end of menstruation and parturition is 278 days.

"4. That the intervals between insemination and parturition, and between menstruation and parturition, have no standard length, but vary within certain limits.

"5. That while absolute proof of the prolongation of real pregnancy beyond its usual limits is still deficient, yet there is evidence to establish the probability that it may be protracted beyond such limits to the extent of three or four weeks, or even longer."

In concluding this brief review it is hardly necessary for us to express the high opinion we have formed of the work before us. By its publication Dr. Duncan has rendered an important service to medical as well as to statistical science. He has, moreover, set us a good example how to rightly conduct statistical investigations, so as to avoid the shoals and quicksands which beset the explorer in these regions. We are not speaking too strongly when we affirm that this, in truth, is an original and philosophical work, and gives abundant evidence of deep thought, severely logical exactness, and patient industry. On some of the important questions to which his researches refer, the data are scarcely sufficient in quantity to render the accuracy of his conclusions unimpeachable; but the author admits and regrets this, whilst telling us they were the only data available for his purpose.

On the Pathology and Treatment of Albuminuria. By William H. Dickinson, M.D., Cantab; Fellow of the Royal College of Physicians; Assistant Physician to St. George's Hospital, and to the Hospital for Sick Children.

The valuable contributions to renal pathology, already published by Dr. Dickinson, have made his name favourably known to all who are conversant with the recent literature of this subject, and accordingly a systematic work on an important class of renal diseases from his pen will be received by the profession with considerable interest. The present volume includes the affections which are usually grouped together under the name of Bright's disease, and is undoubtedly a work of great value and importance, containing, notwithstanding the numerous treatises which are in existence on the same subject, much that is original to the author.

After some excellent practical observations on the examination of the kidney after death, the author proceeds to classify renal diseases, accompanied by albuminuria, under three heads:—

1. Tubal Nephritis.
2. Granular Degeneration.
3. Depurative Infiltration.

Tubal nephritis corresponds with the desquamative nephritis of

Dr. George Johnson. It has its seat in the secreting structures of the kidney—the uriniferous tubes—and is essentially a renal catarrh, or morbidly increased cell growth.

Granular degeneration, which corresponds with the small, contracted kidney, originates, according to the author, in the intertubular fibrous tissue, and consists in a morbid increase in this fibrous tissue, followed by a contraction and condensation of the new material. The effect of this contraction is to produce compression of the tubes, narrowing them at some parts, or even completely obstructing them. Fluid accumulates in the distal portions of the tubes, whose continuity is thus interrupted, and in this way cysts are formed. The epithelium, he believes, is quite unchanged, except by the direct effects of pressure. The affection is, accordingly, closely analogous to cirrhosis of the liver. This account of the pathology of granular degeneration differs altogether from that generally taught and accepted. The view entertained by Frerichs, and generally adopted in Germany, is that it is merely a further stage of the pathological change, of which the large smooth kidney is an earlier condition. Dr. Johnson, again, regards a change in the epithelium of the tubes as the starting point of the affection. Dr. Dickinson's opinions on this subject are worthy of careful consideration, and are supported by some admirably executed illustrations. The question will, no doubt, be soon definitely settled by the numerous investigations which are now being carried on with regard to the minute changes in the kidney.

The depurative infiltration is the name employed by the author to express the waxy, lardaceous, or amyloid degeneration, to which the attention of pathologists have been so much directed of late years. Some additional evidence is brought forward to show the frequent connexion of this condition with the existence of long-continued suppuration. The change in nomenclature has been proposed by the author in a paper on this subject in the last volume of the *Medico-Chirurgical Transactions*, and is advocated by him on the ground that the new name points directly to the suppurative process as being the most frequent source of the disease. We cannot congratulate Dr. Dickinson on his addition to the already abundant synonyms of this affection. Nothing but the most incontestable advantages in scientific precision or in convenience can, in our opinion, justify the change of a well-known name. The proposed term does not suggest to an ordinary reader, when read without Dr. Dickinson's explanation, the meaning which he wishes to attach

to it, the word depurate being in not infrequent use in a widely different signification. The connexion between the amyloid change and the suppurative process is, besides, by no means so invariable as to establish the one as the only cause of the other—indeed, Dr. Dickinson does not assert this; and, finally, the name is based on a principle which is totally at variance with those on which the nomenclature of the other two forms has been founded. Tubal nephritis is so called from the microscopical structure which is the seat of the inflammatory process; granular degeneration from the rough external characters presented by the altered gland; and the new name is founded on something different from both—namely, a pathological theory as to causation, a theory which cannot be, as yet, regarded as proved. The fatty kidney is not erected into a distinct variety by the author, and, we think, rightly. He regards it with justice as a condition engrafted on a previous affection, and is inclined to connect it with cases the result of cold.

The three forms which we have mentioned are described in succession, each of them being the subject of a separate and detailed description, which embraces the pathology, the clinical history, the etiology, and the treatment. A large number of cases are detailed to illustrate the different points advanced by the author. Our limits will not permit us to do more than touch upon some of the numerous questions which are treated by the author. The acute stage of tubal nephritis, such as follows scarlatina, for instance, is recommended to be dealt with in the following manner:—

"The object to be attained is simply to cause the passage of fluid through the kidney, while at the same time we avoid the use of such drugs as would add to the existing irritation. Of all diuretics water is the most valuable. The patient may be restricted to a fluid but nutritious diet, while pure water is taken freely. In children, when the kidney responds readily to this simple stimulant, the disease will generally recover without further treatment. In grown persons, or in children when the disease is severe, digitalis is a most valuable adjunct. Of all drugs this one is of the greatest value in the treatment of nephritis, and was indeed first used in medicine as a diuretic in renal dropsy. If the dropsy be excessive, and the urine correspondingly scanty, it is well to await a certain amount of diuresis by its means, increasing the fluid, which the patient takes in a gradual manner, as the kidney becomes able to deal with it. I believe the best preparation of digitalis is the infusion; the doses may vary from one to four drachms, according to the age of the patient, repeated twice or

thrice in the day, or, in a severe case, as often as every three or four hours, until the urine has become increased in quantity. The purest water should be used; patients do not object to distilled water. Two, three, or four pints a day may be taken, according to the age and other circumstances."

He regards the ordinary mode of treatment by purging with compound jalap powder, and acting on the skin by vapour or other baths, as positively injurious, as "whatever good may be done by way of relieving the gland of its work, is more than counterbalanced by the evil which results from the misappropriation of the aqueous fluid which is needed to keep the tubes clear."

We are strongly of opinion that the basis on which this treatment is recommended is unsound. It is in the highest degree unsafe to found modes of treatment on purely theoretical considerations, even admitting their plausibility; but here the theory seems to have little to recommend it. The great diminution in the secretion of urine in the early stages of the disorder leads directly to a great and morbid increase in the amount of water in the system, as is abundantly evidenced by the anasarca which supervenes; and we are at a loss to comprehend how the ingestion of a few extra pints of water can assist in unloading an already overburdened system. We think that both theory and practical observation concur in pointing to the desirability of endeavouring to remove some of this superfluous water, with, if possible, some excretory matters in solution in it.

The method of relieving anasarca by making incisions with a lancet for the purpose of allowing the dropsical fluid to drain off—a method which is frequently employed—is stated by the author to be very likely to lead to troublesome sequelæ. He regards acupuncture as preferable, and quotes a case in which a needle was used to puncture one leg of an anasarcous patient, while a lancet was employed, at the same time, to puncture the other. The openings made by the needle healed without difficulty; those in which the lancet had been used gave rise to severe suppuration.

An important practical question is discussed, bearing on the selection of the particular surgical operation which should be performed in cases in which there is reason to fear that the amyloid degeneration has set in; for instance, where profuse suppuration has lasted for some time. Under such circumstances, the author believes, that excision should not be performed in consequence of

the exhausting suppuration likely to ensue; but that amputation should be resorted to as the safer expedient, and as less liable to this source of danger.

The author has brought together some curious statistics with regard to the effect of climate on the production of Bright's disease. These are deserving of attentive consideration, and we have no doubt that efforts in this direction will ultimately lead to much valuable information. At present we can scarcely attach much importance to the returns of renal disease furnished by the Registrar-General. The statement made by Dr. Roberts that the number of deaths reported as entered under nephria, which in 1852 was 570, had in 1861 risen to 1,448—an increase quite disproportionate to the increase of the population—is alone sufficient to warn us against basing any inference on these figures. We find it, we confess, impossible to regard the return of a mortality from renal disease of 1 in 49 in Aberdeen, and of 1 in 142 in Glasgow, as representing real proportions.

Some facts brought forward with regard to the influence of alcohol as a cause of Bright's disease are remarkable. Dr. Dickinson has examined the records of the autopsies of fifty-two persons who had died from delirium tremens in St. George's Hospital, and he has found that the proportion of kidney affections in these was only slightly greater than that which was found in the *post mortem* examination of the bodies of the same number of persons who had died from other causes, and in whom death had not been, as far as was known, preceded by intemperance.

The chapter on the pathology of the depurative infiltration contains a summary of the author's views on the amyloid degeneration, as we prefer to continue to name it. The conclusion at which he arrives is that the amyloid substance is fibrin deprived of alkali, and that this deprivation is brought about by a drain of alkali from the system by suppuration. These conclusions are advocated at length in the paper published in the *Medico-Chirurgical Transactions*, to which we have already alluded. It is there argued that the serum of pus contains a much larger amount of alkali than is contained in the serum of blood, and that consequently the discharge of considerable quantities of pus must, when long continued, drain the system of alkali. When we examine the evidence brought forward in support of this view, it seems to us, that the author has fallen into the error of regarding all the salts of the alkalies as possessing actively alkaline properties. He gives the

amount of the bases, without reference to their state of combination, as giving rise to a more or less alkaline condition of the fluid in which they occur. In his own analyses he estimates the total amount of potash and soda without informing us of what acids are combined with them; and he quotes from Simon's *Chemistry* an analysis by Nasse of the serum of pus and of that of blood, in which the amount of alkaline salts contained in the former is stated as 15·32 in 1,000 parts, while the amount in the serum of blood is given as 7·1. On turning to this analysis we find that of the 15·32 parts contained in pus no less than 12·60 are constituted by common salt or chloride of sodium—a thoroughly neutral salt, and one devoid of the smallest alkaline reaction; while the only salts possessing the slightest alkalinity—meaning by the term power of neutralizing acid, or of restoring the blue colour to litmus—which are stated to be present, are the carbonate and phosphate of soda, of which the amount in pus is given as 2·54, while that in blood-serum is stated as 2·30, the difference being little more than two-tenths. We need scarcely observe that all the chlorides and sulphates of the alkalies might be removed from the body without producing the slightest change in the alkalinity of the system. But while we cannot accept the statements put forward as sufficient evidence of there being any considerable diminution of the free alkali of the blood, a change which it would require the direct evidence of numerous analyses of that fluid itself to establish, the following facts which the author adduces as to the character of the deposit itself are very remarkable, and if verified by further researches will throw much light on the pathology of this interesting change:—

"1. The morbid (amyloid) deposit loses its characteristic reaction when it has been allowed to absorb potass or soda.

"2. Organs containing this deposit yield, on analysis, a smaller proportion of the alkalies than do the same organs in a state of health.

"3. Ordinary fibrine or albumen can be made to exhibit all the peculiarities of amyloid tissue by depriving them of alkali by artificial means."

The symptoms and secondary affections of the three forms of Bright's disease are thus discriminated:—

"Tubular nephritis is accompanied more frequently than are the other forms, by pain in the loins, hematuria, dropsical affections, pneumonia, pleurisy, peritonitis, and croup. Uremic nervous affections with a tendency to convulsions are frequent in this variety.

"Granular degeneration is especially associated with gout, with affections of the heart and blood-vessels, and with hemorrhagic affections Convulsive seizures are of frequent occurrence, but there is a still greater liability to simple coma.

"The amyloid disease is marked by diarrhea, which points to the extension of the degeneration to the mucous membrane of the alimentary canal."

We have in this notice dwelt chiefly on the debatable points in Dr. Dickinson's work, and in this way may have unintentionally and unwillingly done him injustice. It is impossible not to be impressed by the marks of earnest and careful work which every chapter bears. The descriptions of morbid appearances are most truthful and accurate, and the illustrations are of great fidelity and beauty. We regard the work as a valuable addition to the literature on the subject.

The Diseases of the Ear; their Nature, Diagnosis, and Treatment. By Joseph Toynbee, F.R.S., late Fellow of the Royal College of Surgeons of England; Aural Surgeon to, and Lecturer on Aural Surgery at St. Mary's Hospital; Aural Surgeon to the Asylum for Idiots, &c., &c. *With a Supplement* by James Hinton, M.R.C.S.; Aural Surgeon to Guy's Hospital. London: H. K. Lewis, 136, Gower-street. 1868.

This is a further issue of the original edition of the late lamented Mr. Toynbee's work, to which is added a supplement by his friend and pupil Mr. Hinton. The original work was published first in 1863, and is probably already known to our readers; we therefore now allude merely to Mr. Hinton's supplement, which is "a brief statement of the advances which have been made since the publication of Mr. Toynbee's work," and some annotations on the text.

Amongst the aids to diagnosis of the conditions of the meatus and membrana tympani, Mr. Hinton recommends the reflecting mirror brought into general use by v. Troeltsch, and of which an account was given in the February number of this Journal for 1863. Siegler's pneumatic speculum is also recommended. This consists of a speculum, the large orifice of which is closed by a slightly magnifying lens; at one side is an opening, to which is fitted an

elastic tube; when the speculum is in the meatus it is air-tight, and suction can be made through the tube on the membrana tympani; "its design was to aid in determining the presence of bands of adhesion in the tympanic cavity, by indicating spots at which the outward movement of the membrane might be impeded. In this respect it has great value; the membrane may be distinctly seen through it to move sometimes as a whole, at others unevenly and in parts."

Some advances have been made of late in connexion with the tuning-fork, especially by Dr. Lucae, of Berlin; this means may be of use occasionally in "distinguishing affections of the nervous from those of the conducting apparatus;" but as yet we think the results of the tuning-fork are not so satisfactorily established as to be able to build up a system of diagnosis upon them; it is probable that it may yet occupy a foremost place in the art and science of diagnosis.

We can fully endorse Mr. Hinton's caution to avoid instrumental interference, and his advice to use the syringe alone in the removal of foreign bodies from the meatus.

We are all familiar with the troublesome nature of boils; when they occur in the meatus they are most distressing; the author has observed them very frequently during the treatment of chronic inflammatory affections of the tympanum, especially perforation of its membrane, and he considers that they are more likely to occur "when the deeper lesion is advancing towards a cure, so that their presence might almost be regarded as a favourable sign." These boils are very apt to recur, and the author recommends the white or red precipitate ointment as a local application, and deprecates incisions; "the use of the knife does not appear to diminish the tendency to recurrence." Sir William Wilde's snare—undoubtedly the best instrument for the purpose—armed with jack-line, is recommended for the removal of polypi; the author's experience of polypi is, that, as a rule, "they spring from within the tympanum;" in opposition to Mr. Toynbee's practice, he advises the removal of polypi "when disease of the brain is threatened from irritation within, or in the neighbourhood of the tympanum."

Several pages are reprints from the transactions of the Medico-Chirurgical Society of Mr. Toynbee's interesting papers on molluscous or sebaceous tumours; these tumours vary in size from a millet seed to a large hazel nut; they increase equally on all sides, and produce absorption of the surrounding bone to so great an extent

sometimes that the tympanic, cerebral, or cerebellar cavities are opened; they produce also enormous dilatation of the meatus, and terminate sometimes fatally; "their progress would seem to be more impeded by the presence of membrane than by bone;" in one case "the tumour went through the external, and then the internal osseous walls of the mastoid cells, and came into contact with the dura mater; instead of penetrating it, the tumour passed upwards, causing absorption of the posterior, and then of the upper wall of the petrous bone, and eventually entered the tympanic cavity without producing any perforation in the dura mater."

The greatest advance made since the publication of Mr. Toynbee's work in 1860, is the art of inflating the tympanal cavity introduced by Politzer, of Vienna. "Starting from Mr. Toynbee's discovery that the [Eustachian] tube is opened by the palate muscles during the act of swallowing, Politzer selected the moment of swallowing to introduce a blast of air into the upper part of the pharynx. This is to be accomplished by means of an elastic bag, provided with a flexible tube. The tube he introduced into the patient's nostril, and bidding him swallow, forcibly compressed the bag at the same moment. The patient swallows readily if he previously takes a little water into the mouth, and the nose being closed over the tube, the compressed air exerts a strong pressure upon the mouths of the Eustachian tubes at the same moment that their muscles tend to render them patent." "The surgeon may simply blow through a piece of elastic tubing, or may employ an elastic bag, provided with a valve, by means of which a succession of quick currents of air can be kept up while the patient drinks by small mouthfuls." By this simple means air is readily forced into the tubes, and obstructions, when not of great magnitude, are overcome; it yields also in connexion with the otoscope and the character of the sound, what some regard as valuable diagnostic aid. We ourselves entertain no doubt as to the diagnostic, and occasionally the therapeutic value of this procedure.

In reference to obstruction of the Eustachian tube, Mr. Hinton says, "the increased concavity of the membrana tympani, stated by him [Toynbee] to be characteristic of such obstruction, is not always present except in children;" the membrane may appear, he says, in adults almost normal, or may have a partially convex aspect. Toynbee believed that during the opening of the tubes air might "either enter or recede from the tympanic cavity;" he also held that air entered during deglutition while the nostrils are

closed, and passed out again during the same act with the nostrils opened. Mr. Hinton, however, remarks, "the act of swallowing, whether the nostrils be closed or not, does not increase the pressure of air in the tympanum, but diminishes it; and it is therefore the suction of air out of the tympanum, not the forcing of air into it, which is heard through the otoscope during that act;" this view he supports on observations on the membrana tympani during the act of swallowing, and on Politzer's manometer experiment. If air is expelled during each act of deglutition, the question naturally arises, when and how does the air find its way into the cavity.

The mode of action of the artificial membrana tympani was long a moot question, Mr. Toynbee, whose name is associated with it, believing it to act by simply closing the aperture; it is now, however, placed almost beyond doubt, by Lucae's experiments and observations, that its effect is produced by pressure, primarily on the ossicula, or their remains, and remotely on the labyrinthine fluid; Mr. Toynbee, during the latter years of his life, came to this view. We regret that Mr. Hinton has passed over this very interesting subject almost in silence.

Catarrh of the tympanal cavity, with perforation of the membrana tympani, is an exceedingly tedious and troublesome affection, for which the author recommends that the patient should himself force air into the tympanum by Valsalva's experiment, or have it done for him by Politzer's method, so as to thoroughly cleanse the parts, by freely mixing air and the astringent lotion, previously introduced through the meatus. After thoroughly cleansing the parts by syringing and air douche, and then drying the meatus, Mr. Hinton largely employs powdered talc, which he blows into the meatus; the action of this powder is, he says, to keep the mucous membrane dry.

The old operation of puncturing the membrana tympani for various morbid conditions has been revived, but according to Mr. Hinton and others, without any beneficial results.

Amongst the advances made of late years in general medicine and surgery, has been the diagnosis of hereditary syphilis, established by Mr. Hutchinson. The disease attacks the ears as well as other organs, and produces sometimes almost complete deafness; its pathology has not as yet been properly ascertained; at Guy's Hospital the disease has furnished more than one-twentieth of the aural patients.

Mr. Hinton has added an index to the work, the want of which in the original issue we ourselves have experienced.

The advances made in aural surgery since the publication of Toynbee's work in 1860, have not been very great or numerous; it is remarkable that while ophthalmic surgery has made such giant strides of late years, aural surgery has advanced so little; this is attributable to the fact of the innermost parts of the eye being now exposed to our view by means of the ophthalmoscope, whereas the innermost parts of the ear and their diseases remain still in comparative darkness and obscurity. Indeed we might say that very little progress has been made, further than what was known in 1853, when Wilde's *Aural Surgery* appeared. It is possible that inflation of the tympanic cavity, catheterization, and the tuning-fork, may yet lead to very important and practical results, and we would, with Mr. Hinton, recommend their employment.

We cannot conclude this notice without expressing our deep sense of the great loss medical science has sustained in the death of Mr. Toynbee; in seeking means to alleviate the sufferings of others, he unfortunately became the victim of his ardent pursuit of knowledge, and lost his life while experimenting on himself. He has, however, left a museum and a reputation behind him which will long keep his memory fresh amongst us. We are glad to find his work continued by one so like himself in zeal, laborious study, and scientific attainments, as Mr. Hinton has proved himself to be.

Clinical Illustrations of Various Forms of Cancer, &c. By Oliver Pemberton, Surgeon to the General Hospital, Birmingham. London: Longman. 1867. Imperial quarto. pp. 128. Highly illustrated with woodcuts and lithographs, plain and coloured.

This beautiful book comes appropriately at Christmas time to gladden the heart of the reviewers. In the quality of its externals—paper, printing, and exquisite illustrations, there is nothing wanting to satisfy the most luxurious book-fancier; while those who go below the surface will find in it a record most valuable, of the experience of an able surgeon. This is its special character. It neither is, nor aims to be, a scientific monograph on the subject of Cancer. It does

not go into its microscopic pathology, nor into the various theories on its nature and origin, which have emanated within the last twenty years from the French, German, and British schools. With the exception of one solitary reference to Lébert, the names of those who have laboured in this field are not alluded to, and this of set purpose, as the author tells us in his preface. The book is what it is meant to be, simply a record of a very considerable number of cases of malignant disease, which have fallen under the notice of the author during the labours of many years, preceded by a few brief general comments.

Mr. Pemberton's pathological creed on the subject of cancer may be summed up in the one sentence, with which he concludes his opening chapter—"The terms malignant disease and cancer are synonymous, and in all respects co-extensive." Some will agree with this definition—more especially the older school—who pride themselves on being practical men, and who ignore what they call the hair-splitting of pathological students; others will probably hold that the great work of the microscopists of our day has rendered this broad use of the term malignant as co-extensive with cancer scarcely tenable. Following, however, the lead of Mr. Pemberton, we shall not here enter into these moot points. His work is eminently practical, and steers clear of all these questions, which, however useful and captivating, must, in justice to him, be laid on one side while we consider his work.

Mr. Pemberton's style is brief, clear, and intelligible. In a few pointed sentences he brings out the characteristics of each group of tumour, not repeating himself, and omitting nothing of real importance. Having briefly laid down the characters, distinguishing malignant from benign growths, and those of the various varieties of cancer, he passes on to detail cases illustrative of cancer in various localities. First and most prominent is scirrhus of the breast, of which about ten cases are detailed as types of the varieties, from the atrophic scirrhus to cancer of the male breast; of this last Mr. Pemberton was so fortunate as to see four examples. The three of which full particulars are given, were undoubted instances of scirrhus. In the course of twenty-five years we have only met one case—it also was scirrhous cancer.

The results of our author's cases of scirrhus are thus summed up —"Of the causes of scirrhus (*sic.*) cancer of the breast, very little is known with certainty. Of forty-eight cases in which I have investigated the early history, six were referable to the effects of external

violence. Eight were believed to have originated in consequence of breast abscesses, whilst in thirty-four the disease had appeared without any known cause.

"The hereditary transmission of scirrhus cancer of the breast from family to family, and especially in the female line, is a circumstance in the history of the disease that has taken strong hold on general, as well as on professional belief. I find, however, that in seventy cases the previous existence of cancer in the family was unknown; in twenty-two of the remaining forty-eight there were but six cases in which any hereditary influence could be traced.

"It appears to me to occur more frequently amongst the married than the single, for out of fifty cases in which I have investigated this point, forty-five were married women, and only five single."

His principles for operation are sound, but as they contain nothing at variance with those in ordinary acceptation, we shall not need to quote them at length. In some of his cases he has been fortunate in having an interval of many years between the operation and relapse; but, on the point of relapse, or on the average duration of the disease with or without operation, we have no statistics. The immediate results of operation have been three deaths in thirty cases; one in seven days; a second in three weeks, of inflammation of the pleura; and one at the end of six weeks, of exhaustion. This average of ten per cent. would be considered high here, although probably not higher than one would expect in an English hospital, the constitution of the English, and their propensities in the matter of feeding heavily, being unfavourable for all operations, when compared with the sparer habit and diet of the Irish. We venture to affirm that so great a primary mortality after amputation of the breast, is quite unheard of in Dublin, while Mr. Pemberton's comment on it is, that the operation is very seldom fatal to life. The observations on after-treatment are few, but judicious. He approves of generous diet, and of the persistent exhibition of arsenic as a tonic and preventive; we have used it frequently, but without perceiving any benefit in the majority of cases. In addition to opiates, quinine in large doses is recommended for relieving the severe periodic pains of the latter stages of the disease.

In the chapter on Scirrhus of the Rectum, there is a good decription of Amussat's operation for colotomy, with the reasons which should lead to its performance.

Passing over an interesting case of scirrhus of the thyroid gland, we come to encephaloid tumours of the cranium; of this class of

cancers a most interesting example is given, in which the tumour grew slowly from the cicatrix of a wound. Eleven years elapsed before it reached the size of a hen's egg, and three more before the fatal termination. Two large lithographs illustrate the development of the disease, and its rapid growth at the last, while a woodcut shows the condition of the bone after maceration. Of its size just before death put an end to the patient's sufferings, the following extract will give some idea:—

"The tumour grows with extreme rapidity, and now rears itself directly upwards for about seven inches above the level of the skull, overhanging the brow, and exceeding in dimensions the size of head and face together. The tumour had a considerable bony basis in addition to its softer portions, and it pressed upon the brain; the only internal organs affected were the lungs and bronchial glands. Death took place from repeated hemorrhage and sloughing."

As a point of diagnosis between cancerous and benign growths of a warty origin on the scalp, Mr. Pemberton gives as the result of his experience, that the latter may generally be recognized by the presence of more or less healthy integument, and by the growth of hair upon them. The point is worth noting for further investigation.

In the chapter on Encephaloid of the Upper Jaw, a case is recorded where the disease began and reached a degree of development which called for removal of the bone, the patient being all the time covered with a well-marked syphilitic eruption. That the tumour was cancerous, was proved by examination at the time of its removal, and by examination upon the death of the man, four and a half years after the operation. It is impossible to detail all the interesting cases in this volume; they are all well selected, and their story is well told. We must refer our readers to the book, especially for some remarkable instances of adenoid tumours of the breast, and one of proliferous cyst, with recurrence of the growth. Mr. Pemberton seems to have a singular fortune in meeting with enormous tumours of all kinds.

In the chapter on Encephaloid of the Testis, we find the following observation:—"The inguinal glands are not unfrequently enlarged in scrofulous or tubercular disease, whereas it is very unusual to find them affected in cases of encephaloid of the testicle." The lumbar glands seldom escape in cancer of the testis, and we doubt if

the inguinal are not, as a rule, affected sooner or later. They certainly become so, the moment the disease extends from the testis and its proper covering, to its outer tunics. We must pass over, without notice, the remaining valuable chapters which treat of cancer in connexion with bone, melanosis and epithelial cancer in various localities.

The last chapter deals with rodent ulcer, a name given by many English surgeons indiscriminately to lupoid and epithelial ulcers, as well as to what we know here as Jacob's ulcer. Mr. Pemberton gives a definition of rodent ulcer, which shows that he at least understands the difference. He limits the term to the fibro-plastic or Jacob's ulcer; not that he uses either of these terms, but that his definition applies to this alone. This is as it should be, for the confusion engendered some few years ago by the indiscriminate application of the term in the London journals was perfectly hopeless. No statistical results of any value can be obtained, so long as people go on grouping in one class diseases of essentially different nature. This is one grand reason for separating the statistics of epithelioma from those of scirrhous and encephaloid cancer. Whether it is to be looked upon as cutaneous cancer or not, its rate of progress, and its curability by operation, are so essentially different from the other two, that all statistics, in which it is confounded with them, are vitiated and fallacious. For this reason, if for no other, we think those writers have done good service who have insisted on dissociating it from cancer. Mr. Pemberton does not enter into this vexed question, nor does he attempt to generalize much from his cases, which, though sufficiently numerous to give him a fair basis of rule for personal guidance, are necessarily quite too few for the purposes of statistics, unless combined with similarly careful records from the hands of other observers. In conclusion, we express a hope that the profession may, at some future time, be favoured with further records of Mr. Pemberton's experience in other branches of surgery. Honest and plain records, such as those in the pages before us, are the material, of all others, most valuable, whether to the hard-working student of pathology, or to the hard-worked practising surgeon. And how much more agreeable are they when they come to him in handsome garb, such as Mr Pemberton has not grudged to his *Clinical Illustrations.*

Orthopædics: A Systematic Treatise upon the Prevention and Correction of Deformities. By DAVID PRINCE, M.D. Philadelphia: Lindsay & Blakiston. pp. 240.

Club-foot: Its Causes, Pathology, and Treatment; being the Essay to which the Jacksonian Prize for 1864, *given by the Royal College of Surgeons, was awarded.* By WILLIAM ADAMS, F.R.C.S. With 100 illustrations on wood and stone. 8vo, pp. 422. London: John Churchill & Sons.

THE works, whose titles appear at the beginning of this notice, present features of marked contrast in more respects than one.

The first is a treatise prepared, according to the author's preparatory statement, with special reference to the wants of physicians engaged in general practice. It is divided into two parts; the first treating of deformities generally, from squint down to the cramped feet of Chinese ladies, and the constricted waists of western beauties; in the latter part considerable space is devoted to the consideration of diseases of the spine and club-foot. The mode of arrangement, and the manner in which the subject matter of the book are dealt with, are alike exceedingly faulty, and the quotations and extracts from other authors, though in many instances excellent in themselves, are so numerous as to form a large portion of the book.

In the treatment of diseases of joints, resulting in deformity, the author adopts the practice of gradual and continuous extension as being the one followed by the best results. The necessity of first awaiting the subsidence of all signs of active inflammation in the affected joints, is, of course, insisted upon, and the writer properly recommends that the extending force should be primarily applied in the direction in which the limb has been left by the disease, and that no effort should, in the first instance, be made to alter the angle of flexion:—

"The neglect of this very important distinction has doubtless led to many failures, when success was practicable. It applies to all extremely flexed joints, especially to the hip, to the elbow, and to the shoulder; and it may be succinctly stated as a rule, *to extend the tendinous and ligamentous fastenings first and change the angle afterwards.*

"It will be easily seen, that an extension in the direction of the acquired deviation may be borne, which could not be tolerated if applied directly

to correct the malposition, on account of the extreme pressure of joint surfaces upon each other, which must be produced by the long leverage of the bone attempted to be reduced to position."

In the second part of this book, different forms of the complicated apparatus used for the correction of spinal deformities are depicted, and some simple modifications, devised by the author, described. The section on Talipes is, perhaps, the best part of the book, although it is also very faulty. Dr. Prince seems to have adopted, almost without an exception, the views of Mr. Barwell, of London, as to the nature and causation of the disease, and in illustration, gives paragraph after paragraph from that gentleman's book, entitled, *On Club-foot, without Cutting Tendons.* Dr. Prince describes a simple, and, we have little doubt, an efficient appliance in mild cases, for removing the deformity in talipes varus. It is constructed principally of elastic cords, attached from the knee above to a piece of gutta percha carefully moulded on the foot. The book is well illustrated, contains some valuable matter, and whatever is derived from other sources has been honestly acknowledged.

The volume we have next to notice is a monograph of a very different description from the last. The causes, pathology, and treatment of club-foot receive, at the hands of the Surgeon of the Orthopædic Hospital, the fullest and most scientific consideration.

In the course of twenty chapters, with an appendix of cases, of this admirably illustrated book, everything at present known, or, at all events, worth knowing, is told of deformities of the feet, whether congenital or acquired.

It is unnecessary, perhaps, to enter into any lengthened analysis of the contents of the second edition of a Jacksonian Prize Essay, a name which speaks for itself. We may, however, be permitted to refer, in the first place, to the interesting historical summary in the opening chapter, in which the author takes the opportunity of correcting some errors of other writers. The chapters devoted to the anatomy and pathology of the disease are very full, and the great importance of fully distinguishing congenital from non-congenital cases dwelt upon, especially as concerning the prognosis and the treatment. Mr. Adams joins issue in no undecided manner, both with the principles and the practice advocated by Mr. Barwell in the book already mentioned. He also refers, with marked disapproval, to the method adopted by Mr. Syme, and other surgeons of

the Edinburgh school, who affect, the author states, to consider a club-foot cured, if the deformity has been remedied by section of the tendons. In fact, in reference to what may be styled the "immediate treatment" of club-foot, we thoroughly coincide with the views expressed in the following extract, except in mild infantile cases:—

"It is assumed, as an established fact, that the after-treatment should be most carefully conducted by the aid of mechanical appliances, although the contrary opinion may still be held and publicly taught by Professor Syme, of Edinburgh, who, after dividing the tendo Achillis, even in paralytic cases, allows the patient to walk on the second or third day. How can it be imagined that the reparative process in the divided tendon is sufficiently advanced on the second or third day to admit of exercise of the joint being resumed with advantage? Yet Mr. Syme observes that after the operation 'the bandage will be kept on for forty-eight hours, when no further treatment will be required, as the use of the limb in walking will be sufficient to bring the foot into its natural form,' and he relates several cases in illustration of this practice. However, the serious results which follow this procedure have been previously adverted to, such as non-union, or an extremely weak and elongated condition of the tendon, rendering the foot much less useful to the patient than before, and hopelessly incurable."

It is upon the proper combination of mechanical, physiological, and operative means, that Mr. Adams relies for obtaining permanently successful results. With reference to the period for performing tenotomy in infantile cases, the author considers that as early as possible after the first month, as the circumstances of the case will permit, is the best. He finds, practically, that the most favourable time is when the child is about two months old. In this Mr. Adams is opposed to the experience of Dr. Little, and of many surgeons, who consider that just before the child commences to walk is a more suitable time. For the arguments, *pro.* and *con.*, we may refer our readers to the book itself. The author lays great stress upon the order in which the tendons should be divided, considering it bad surgery to divide the Achilles' tendon at the same time as the rest, as thereby the fulcrum, or point of resistance, is lost, upon which you exert the force necessary in unfolding the foot at the medio-tarsal joint, besides other disadvantages which are thus referred to—

"There is no greater error in the treatment of severe varus, or one more frequently committed by surgeons of the present day than the

division of the tendo Achillis at the first operation, either by itself, or conjointly with the division of the other tendons. By such a procedure, too many objects have to be accomplished at the same time by mechanical means, and failure in some of them usually results; eversion of the foot is imperfectly obtained, depression of the os calcis is also imperfectly obtained, and the recurrenee of the deformity rendered certain. It has long therefore been an established rule in Orthopædic practice that the tendo Achillis should be the last tendon divided, and that its division should be delayed till the equinus alone remains to be cured."

In reference to another important practical matter—the duration of the treatment, and the mode of regulating it—Mr. Adams has shown, by experiment, that tendons after division are united by new material, almost precisely similar in appearance and structure to the old. He, therefore, always allows a few days to elapse before commencing the mechanical treatment, in order to allow this new matter time for partial organization; adding, that this interval, and the rate of extension afterwards, should, in every case, be regulated by the presumed activity of the reparative powers of the patient. In conclusion, we may say of this treatise, to some of the more salient features of which we have thus alluded, that it is a work which, whether from a scientific or a practical point of view, does honour to the author, and is an acquisition of much value to the province of surgery.

Parturition and its Difficulties. By John Hall Davis, M.D. London: Hardwicke. 1865. Small 8vo, pp. 354.

The former edition of this work received a favourable notice at our hands (Vol. xxvii. of this Journal), and we are happy to be able to bestow the same praise on this, the second edition. The first thing about it that strikes our attention is a change of title. It is no longer "Illustrations of Parturition;" nevertheless, we think this title gives a more correct idea of the subject matter of the book than does the new one, which to our minds is too comprehensive; many of what may be called "difficulties," (*e. g.*, ruptured uterus, convulsions, plural births, &c.) not receiving any consideration. However, we are told, in the conclusion to Part I., that in a future volume, which will appear shortly, the author intends to treat of the complications of labour.

The work before us consists essentially of three parts—Part I.,

occupying 145 pages, contains the observations and opinions of the author in regard to the different varieties of difficult and preternatural labour, together with a description of the mechanism of parturition. Part II. brings before us the clinical histories of 152 cases of difficult and preternatural labour. Part III. gives a general summary of the statistics of 13,782 deliveries under the author's superintendence. There is one serious defect in these statistics, they do not give the proportion of first and subsequent labours, which greatly lessens their value and utility. The low rate of mortality among the patients of the Royal Maternity Charity, viz., 1 in 444, would seem to indicate St. Paul's Cathedral as the centre of a district which deserves to be regarded as the Utopia of lying-in women.

Dr. Davis still holds to the opinion that, *cæteris paribus*, the forceps operation is more dangerous to the mother than that of craniotomy, and, therefore, he enjoins that the forceps should never be used for the delivery of a dead child. We are satisfied that on this point—the comparative danger of delivering by the forceps, and by the crotchet—Dr. Davis is right, and that very erroneous notions have prevailed, and have had the support derived from a skilful but illogical use of statistics. We think it can be demonstrated that where the two operations have been performed *under like circumstances* as to the duration of labour, nature of the difficulty, and previous attempts at interference, craniotomy cases show a better result than do the forceps cases, thus incontestably establishing the point Mr. Davis stands out for.

Of the *cephalotribe* (about which we heard so much at the recent meeting in Dublin, of the British Medical Association), the author does little more than mention it by name, and that merely in a foot note, and in a deprecating sort of way. On this point, however, the author will probably alter his opinion, as the instrument, in some of its recent modifications, is likely to prove a real and valuable addition to the mechanical resources of the accoucheur.

The comparative advantages of the long forceps, and of turning (as originally recommended by Sir F. Ould), in cases of moderate pelvic deformity, are not yet finally agreed on amongst obstetric authorities. Towards the decision of this point we have the statement of Dr. Davis, that on at least three occasions, where the long forceps had utterly failed to accomplish the delivery, he succeeded in performing version, and in saving one of the children and all the mothers.

He gives, however, this very salutary caution, that, if the turning be not pretty easily accomplished, we must for the greater safety of the mother relinquish it and resort to craniotomy.

The extensive collection of clinical histories, recorded in the second part of the work, is of great value, and will afford much instruction to the thoughtful reader.

On the whole we feel grateful to Dr. Davis for the publication of this work. The opinions and the clinical experience of a man who has enjoyed such extensive opportunities for improvement, must form a real and permanently useful addition to obstetric literature, and we shall anxiously await the appearance of his promised work on anomalous or complicated labours.

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1. *Injuries of the Spine; with an Analysis of nearly Four Hundred Cases.* By JOHN ASHHURST, jr., A.M., M.D. Philadelphia: J. R. Lippincott & Co.. London: Trübner & Co. 1867.

2. *Clinical and Pathological Facts in reference to Injuries of the Spinal Column and its Contents. London Hospital Reports*, Vol. III.

3. *On Dislocations and Fractures of the Spine. A Clinical Lecture.* By JONATHAN HUTCHINSON, F.R.C.S. *London Hospital Reports*, Vol. III.

THE author of the little volume first on our list, and which does credit to the publishers by the manner in which it is got up—is surgeon to the Episcopal Hospital of Philadelphia. In his opening observations he remarks that—

"Injuries of the head have been looked upon with great interest by writers on surgery from the earliest days of our art, and the works of surgical authors, from the time of Hippocrates to the present day, abound in observations and instructions with regard to the management of that class of affections.

"Injuries of the spine, however, whether, as suggested by Sir Benjamin Brodie, because the spine has been considered as a mere appendage to the head, or possibly on account of the confessedly hopeless nature of many of these injuries, have received but comparatively little attention;

and we find the same remarks and the same cases quoted (and not unfrequently, it must be acknowledged, misquoted) from book to book, without any apparent effort on the author's part to ascertain the correctness of his predecessor's observations."

He then refers to the great interest that was excited in the minds of the profession by the controversy between Sir Charles Bell and Sir Astley Cooper, on the propriety of resection in cases of fracture of the spine, and to the fact, that more reliable information on the subject has accumulated since that time; and concludes by offering the present essay as a contribution towards the settlement of the vexed question.

Dr. Ashhurst has examined the clinical histories of 394 cases of spinal injury, and of these he found that no less than 124, or 31·47 per cent. had been reported as instances of pure dislocation; 195, or 49·49 per cent. were alleged to be cases of fracture alone; and 53, or 13·45 per cent. as fractures with dislocation conjoined. He strongly suspects, however—and not, we believe, without reason—that in some of the cases reported as dislocations alone, there must have been bone lesions as well. The relative frequency with which these injuries affect different regions of the spine is given in the following table:—

Cervical region alone affected in . . .	208,	or 52·79	per cent.
Cervical and dorsal regions affected in .	4,	„ 1·02	„
Dorsal region alone affected in . . .	106,	„ 26·90	„
Dorsal and lumbar regions affected in .	20,	„ 5·08	„
Lumbar region alone affected in . . .	37,	„ 9·39	„
Region not clearly indicated affected in .	19,	„ 4·82	„

The great preponderance is in the cervical region, a circumstance which Dr. Ashhurst accounts for on the ground that these cases being too often rapidly fatal, excite a larger share of interest in the mind of the observer. It should not be overlooked, however, that the anatomical arrangement of the elements of the spinal axis would warrant an *à priori* conclusion, that dislocation, if not fracture also, would be more liable to ensue in the cervical than in either the dorsal or the lumbar regions. As might be anticipated, too, with regard to the sex of those who met with injuries of this kind, the large majority occurred in males; and, while no age appears wholly exempt, the greatest number of dislocations and fractures of the spine have been met with during the active period of adult life.

The next portion of the work, devoted to the discussion of the symptomatology, is of considerable importance, and of much practical interest. The accurate diagnosis of the nature and extent of the lesion in cases of injury of the spine is always the point of greatest doubt and difficulty. After enumerating some of those perplexing, and apparently inexplicable cases, where the injury and the symptoms dependent upon it stand in no ordinary relationship, the author refers to the electric test of M. Landry, whereby the extent of the cord lesion in a downward direction may be ascertained:—

"Not only," he states, "can the seat of the cord lesion be ascertained from observation of the parts paralysed, but the extent of the lesion has also been accurately determined before death, by observing what muscles can be made to contract by electricity. Thus M. Landry, in a case of dislocation of the fifth dorsal vertebra, found that the muscles of the thigh ceased to respond to electricity, while those of the leg, though equally paralysed, continued to contract when submitted to the electric stimulus. The autopsy showed that the part of the spinal cord whence arose the femoral nerves was disorganized, while the part below which gave off the crural nerves was quite healthy.

"Thus the fact that each segment of the cord constitutes a separate nerve centre, furnishes a means of most accurately diagnosticating the extent of injury which the cord has received." [a]

Allusion is also made to the occurrence of "flushed face," which the author says has generally escaped the attention of writers on spinal injuries. It has only been observed in cases where the cervical portion of the cord has been seriously affected, and is a symptom of the gravest import. It has been accounted for by M. Topinard by the partial paralysis of the sympathetic nerve, that nerve deriving its cervico-cephalic branch from the "cilio spinal region" of the spinal cord, as demonstrated by Messrs. Budge and Waller. We are aware, in corroboration of this explanation, of the experiments of Claude Bernard, who found that division of the sympathetic nerve in the neck of a rabbit was followed by a marked increase of temperature of the corresponding ear, due to an augmented flow of blood to the part. Alteration of the vital temperature is a symptom which Dr. Ashhurst considers has not yet been sufficiently investigated. He believes, however, that—

"Persistent elevation of the temperature in spinal injuries is a most

[a] Landry: Recueil des Travaux de la Société Médicale d'Observation, Tom. i., p. xvii.

serious symptom, and furnishes grounds for a gloomy prognosis. It gives reason to fear that progressive disorganization of the cord is going on which will almost surely terminate fatally.

"*Diminution* of the temperature in the parts paralysed is, as might be anticipated, not an unfrequent symptom in the course of such spinal injuries as survive the risks of the earlier periods."

In concluding a most instructive account of his analysis of the symptoms of injuries of the spinal cord, the writer, after dwelling upon the extreme importance of being able to say what is the probable amount of injury in any given case, which is, indeed, the only datum upon which to found even the most guarded prognosis, recapitulates as follows:—

"If the paralysis below the seat of injury be immediate, complete, and permanent, there is every reason to fear that the cord has been absolutely divided. If the paralysis be immediate but not permanent, it is probably a case of 'concussion of the cord,' so called, without serious structural lesion. Should paralysis come on after a few hours, and gradually increase to a certain point, then to diminish again, it is probably due to effusion of blood, within or without the cord, which, under favourable circumstances, may be reabsorbed, or coagulated, or may possibly remain in a fluid state without giving further annoyance, as in the case reported by Mr. Heaviside.[a] If, however, the paralysis gradually creep up, every day invading a fresh space, there is scarcely room to doubt that progressive disorganization of the cord is taking place, which, if not spontaneously arrested, will infallibly sooner or later prove fatal."

Dr. Ashhurst disputes the authority of Dr. Brown-Séquard, in placing the proportion of cases which survive severe spinal injuries under the ordinary treatment, "at perhaps less than 1 per 100." A table is given in support of this position, analysing the results in 394 cases, under the following heads:—Those who were sufficiently restored to pursue their former avocations, or to maintain themselves by comparatively active exertions, are classed as recovered; those as relieved, who, though benefited by treatment, could not be fairly considered as recovered; and those as not improved, who, though life was prolonged, yet remained as helpless as they were at the beginning of the treatment. The statistics are certainly sufficiently startling:—

[a] Cooper: Disloc. and Fract. of Joints, p. 545.

Tabular View of the Results of 394 Cases of Spinal Injury.[a]

Results	Cervical Region (212 Cases)	Dorsal Region (130 Cases)	Lumbar Region (57 Cases)	Region not stated (19 Cases)
	Cases. Per ct.	Cases. Per ct.	Cases. Per ct.	Cases. Per ct.
Recovered . . .	38 or 17·92	28 or 21·54	15 or 26·32	8 or 42·10
Relieved . . .	6 „ 2·82	13 „ 10·	6 „ 10·52	3 „ 15·79
Not improved . .	2 „ ·94	6 „ 4·61	1 „ 1·75	3 „ 15·79
Died	164 „ 77·36	82 „ 63·08	34 „ 59·66	5 „ 26·32
Not stated . . .	2 „ ·94	1 „ ·77	1 „ 1·75	... ...

Before proceeding to consider what is the best method of treating injuries of the spine, Dr. Ashhurst gives an interesting review of the opinions of the early writers, from Hippocrates down to more recent times. The advice given is very various and contradictory—some authors recommending the reduction of a dislocated spine by the most violent measures, and others, like Heister, leaving the matter very much "to God and the goodness of Nature." "For slight wounds," he says, "heal quickly, but those which are more severe cannot be cured in any way."

The operation of resection, so far as Dr. Ashhurst has seen, has been performed twenty-five times since Cline first practised it. Louis' operation, performed in 1762, is not regarded by the author as a case of resection at all, merely consisting, he says, of the removal of some loose fragments in a case of gunshot fracture. Elaborate tables are given, too long to quote, showing in a tabular view the treatment employed in the injuries of various regions; an analysis of the nature of the injuries as regards results; an analysis of the results as regards the treatment employed; and a comparison of the results of various modes of treatment in each. From these tables and from the considerations urged in support of his views, the author considers the following conclusions justifiable:—

"1. Injuries of the spine are not nearly so fatal as is generally supposed, and they have been, not unfrequently, completely recovered from.

"2. By watching carefully the symptoms and knowing the lesions

[a] In this table, as in others, it must be observed that cases in which *two regions* of the spine were involved are noted under *each region*.

which they indicate, the patient's progress toward health or death can be pretty accurately foreseen in most cases.

"3. Whenever there is reason to believe that one or more vertebræ have been displaced, extension should be employed: temporary, if that be sufficient; if not, continuous.

"4. In no case do resection or trephining offer a reasonable prospect of improving the patient's condition, but, on the contrary, there is reason to fear that they would increase the chances of a fatal termination.

"5. Those cases of spinal injury which are not adapted for the employment of extension, should be treated in accordance with ordinary rational and physiological principles.

"6. No new mode of treatment is entitled to adoption in a class of injuries so serious as this, unless it can be shown by clinical experience that it is at any rate *not less successful* than the modes commended to us alike by reason and long experience."

The volume, whose contents we have thus analysed, contains very much to interest the practical surgeon. The information it affords is most valuable and instructive, the more so, as it refers to a class of cases which are comparatively rare, thus rendering it impossible for any single individual to meet with a number sufficiently large to warrant a very positive expression of opinion. The question, however, of surgical interference in cases of injury of the spine is as yet open, notwithstanding the strong evidence which the statistics adduced by Dr. Ashhurst afford as to the impropriety of resection. Extension, as practised by the ancients for dislocations, seems in later times to be the most successful method of treatment as regards these displacements, the mortality being nearly four times greater when constitutional treatment has been relied on exclusively, than when attempts have been made to reduce the dislocation by extension or rotation. In concluding this notice it may be fairly urged upon surgeons the desirability of making known any facts bearing upon this important subject, as it is only from a sufficiently large number of well authenticated clinical histories that any just induction can be made.

In the last number of the journal we briefly noticed the two articles on injuries of the spine published in the *London Hospital Reports*, Vol. iii., and we now again refer to them for their bearing on the opinions advanced by Dr. Ashhurst. One of them is in the form of a clinical lecture by Mr. Jonathan Hutchinson, and gives a summary of the clinical experience recently obtained in the hospital regarding displacements at different parts of the

spinal column. His object, he states, is to furnish conclusive evidence in support of the usual practice at the hospital, namely, abstaining from operative interference. In support of his conclusions an account is given in the first of the two articles of some nineteen cases of spinal injury; and it is asserted that while a great many cases, if let alone, recover when placed under favourable circumstances, it is very rare, indeed, that after any *post mortem* it could be alleged that an operation would have done good; and that, moreover, it is impossible beforehand, to tell which are the cases where the surgeon may interfere with advantage. A very important fact is brought out by Mr. Hutchinson, that permanent displacement of the vertebræ to any material extent, is comparatively rare, not happening, he states, oftener than once in ten times. In many of the reported cases displacement must have taken place to the extent of thoroughly crushing the cord, and yet the bones were found, on examination, to have spontaneously resumed their proper places. The writer of the article also considers compression of the cord from the pouring out of blood very uncommon, and that this lesion is often incorrectly invoked to explain the presence of paralysis when no fracture has been discovered.

"Large effusions of blood, whether in connexion with fracture or contusions of the spine are, I believe, amongst the very rarest occurrences. I have never myself seen any effusion to the extent of possible compression, and in the majority of cases there is little or none. The injury is a crush, and is not one at all likely to cause much bleeding. There are no large arteries to be injured. When you examine a specimen of fractured spine, you will find the muscles and soft parts externally much ecchymosed, there will be also a few small blood-clots adhering to the edges of the broken bones, but there will be little or no blood between the bone and the theca, and none at all in the thecal cavity. I speak of what is usual, and am well aware that exceptions may occur. The theca (dura mater) is very rarely torn, and often, on exposing it, you might imagine that the cord was not injured. Nay, even further than this, you find the pia mater of the cord entire and without ecchymosis, and only on slicing the cord through, do your discover that its substance has been utterly smashed. I show you a sketch of a cord thus injured. You will see that the cord-substance, for nearly an inch and a half, is reddened by effused blood, and its substance broken into a diffluent pulp. Yet, in this instance, the pia mater was still entire."

In concluding a necessarily brief notice of Mr. Hutchinson's Lecture, which abounds with practical information of the utmost

clinical value—tending, we believe, very strongly against any attempt being made to treat injuries of the spine by operation—we would wish to call attention to the following extracts:—

"In making a diagnosis, in the first instance, loss of sensation is of much more value than loss of motion, for the obvious reason that the latter may be only apparent. I will venture one hint as regards the diagnosis of fracture from cases of concussion of the spine. It is this, examine the lower extremities carefully as regards sensation, and if you find that at any parts sensation is utterly lost, so that you can thrust pins into the skin, rest assured that there is more than mere concussion. It may be laceration of nerve-trunks, or it may be a crush of the cord, or of part of it, but there is certainly some structural lesion. I am not much of a believer in severe symptoms resulting from mere concussion of the spine, but I feel sure that it never produces absolute paralysis of any part, however small.

"It is obvious that the *prognosis* will depend upon your estimate of two factors. First, the position of the injury, and, secondly, the extent to which the cord has been damaged. First, then, are the cases in which the existence of complete paraplegia below the seat of injury, indicates that the cord has been most severly damaged. In these the danger to life increases in proportion as we ascend towards the medulla. Many, perhaps most, of the fractures in the lumbar and lower dorsal regions, might recover if it were possible to avoid cystitis and bed-sores. There is nothing necessarily fatal in complete and permanent paralysis of the lower extremities and of the sphincters. But, in these cases, you may hope for a better result than this. In not a few cases the paraplegia will disappear and the patient make a complete recovery. You have but to see to his careful nursing, place him on a water-bed, and abstain from serious interference.

"If the fracture be high enough to implicate the respiratory muscles, a new element of danger from pulmonary congestion, &c., is added, and if it be so high as to paralyse all the thoracic muscles, the prognosis becomes exceedingly grave. But very few such patients recover or even survive many days."

The facts of the interesting and instructive case of Spinal Injury, published in our last issue, possess much importance relative to the question in dispute, and should not be passed over. Dr. MacCormac has there conclusively shown how a condition may exist apparently most favourable for the operation, so far as the mere act of trephining is concerned, when the injury to the cord is at the same time irreparable, and much more severe than could reasonably

have been expected. We fear, in short, that the histories of *post mortem* examination in fractures or displacements of the spinal column afford, as a rule, but slight encouragement to operative surgery.

WORKS ON ANATOMY.

1.—*Quain's Elements of Anatomy.* Seventh edition. Edited by Wm. Sharpey, M.D., F.R.S.; Allen Thomson, M.D., F.R.S.; and John Cleland, M.D. In 2 vols., 8vo. Illustrated by upwards of 800 engravings on wood. London: Walton. 1867.

2.—*The Physiological Anatomy and Physiology of Man.* By R. R. Todd, W. Bowman, and L. S. Beale. A new edition by the last-named Author. Part I. London: Longmans. 1866.

3.—*Atlas of Surgical and Topographical Anatomy.* By B. J. Béraud. Illustrated by one hundred plates, drawn from nature, by M. Bion. Translated by Robert Thomas Hume, M.R.C.S., Eng. Complete in eleven parts. London: Baillière. Nos. 3 to 11.

4.—*The Nervous System.* By Ludovic Hirschfeld. Edited in English by Alex. M. Macdougal, F.R.C.S. With artistically-coloured lithographic illustrations, designed by J. B. Léveillé. London: Churchill. Nos. 1 and 2.

5.—*The Convolutions of the Human Cerebrum Topographically Considered.* By Wm. Turner, M.B. Edinburgh: Maclachlan and Stewart. 1866.

6.—*Osteology: A Concise Description of the Human Skeleton, adapted for the Use of Students in Medicine. Accompanied by an Explanatory Atlas of Plates.* By A. S. Norton. London: Hardwicke. 1866.

Quain's *Anatomy* has now reached the seventh edition. The general anatomy has again been revised by Dr. Sharpey, and the descriptive anatomy is now in a great degree recast, under the supervision of Dr. Thomson, by Dr. Cleland, Professor of Anatomy in Queen's College, Galway. The new edition of Todd and

Bowman's *Physiological Anatomy* appears under the editorship of Dr. Beale, who assisted the authors in completing the first edition. The first part now before us, consisting of the *Introduction* and the chapters on *Structure* and *Chemical Composition*, is complete in itself. The reputation of both works is too well established to require comment; they have long been, and most justly so, esteemed as the highest standards on the subjects of which they treat, and we need only say they have not suffered at the hands of their present editors.

In our issue for August, 1866, we spoke in very laudatory terms of the first and second parts of Béraud's *Topographical Anatomy*. The work is now complete, and fully justifies all we said of it. The anatomy of all the important regions of the body is shown in 109 beautifully-coloured plates, so that the relations of the several parts may be seen at a glance. In one plate only have we detected a fault, viz., that which shows the interior of the female pelvis. The uterus is shown ante-flexed, and the vagina as a straight tube, with the os uteri looking directly downwards, a condition of the parts very far removed indeed from their natural state.

Of Hirschfeld's *Anatomy of the Nervous System* we have two numbers, containing five single and four double plates. It is intended to complete the series in twelve numbers, containing 56 single and nine double plates, or 74 in all, embracing the anatomy of the nervous system of the whole body. Judging from the specimens before us, the work will be one of great value, and the English edition will be well worthy of the reputation the work has already obtained on the Continent. Dr. Turner's pamphlet on the *Convolutions of the Cerebrum* will prove a valuable aid to those who are engaged in studying the functions of the different portions of the cerebral mass, a study to which the recent observations on aphasia lend renewed interest.

Norton's *Osteology* used rightly, will, we are sure, prove a useful book for students. We hope it will not be taken as a substitute for a good set of the bones themselves. The atlas consists of twenty uncoloured lithographic plates, in each of which several bones, or different aspects of the same bone, are shown; and instead of mere outline the artist has endeavoured, and very successfully too, to represent the solid bone. He has also placed figures on several points of the drawings themselves, so that there is no difficulty in defining the exact part referred to in the accompanying description.

Illustrations of some of the Principal Diseases of the Eye, with a Brief Account of their Symptoms, Pathology, and Treatment. By Henry Power, F.R.C.S., M.B., Lond.; Surgeon to the Royal Westminster Ophthalmic Hospital; Assistant-Surgeon to, and Lecturer on Physiology at the Westminster Hospital. London: John Churchill and Sons, new Burlington-street. 1867.

This work is an admirable treatise on diseases of the eye, and reflects very great credit both on its author and on its publisher; it is one of the handsomest and best got up medical works which has been issued from the press for some time; we would especially commend its letterpress and binding.

Mr. Power is evidently thoroughly conversant with the foreign, especially the German, literature of the present day, and makes use of it, perhaps, a little to the exclusion of British literature. In this he may possibly be right, as the principal advances which have recently been made have in reality originated in foreign schools. His work is a valuable one, and as a rule a very reliable book of reference to what has been accomplished during the past ten years. The author, so far as his knowledge extends, is most liberal in giving credit to whom credit is due, and in the quotation of authorities; indeed, one of the faults of the work consists, in our opinion, in its too great impartiality—a fault not usually complained of. The author is often very profuse in his quotations of the opinions and modes of treatment adopted by others, but very reticent as to what his own views may be; thus, in the ten pages devoted to the modes of treatment of entropium, trichiasis, and distichiasis, he does not definitely recommend any one method, but leaves us in the dark as to his own practice, unless we are to gather from his statement of his having seen Mr. Guthrie's operation "succeed very fairly," that he carries it out. In this same chapter on entropium, Arlt's modification of Jaesche's operation of transplantation is most unaccountably ascribed to Professor E. P. Wright of this city. No one will, we are sure, be more surprised at this error than Dr. Wright himself, for in his paper on the subject, in the 39th volume of this journal, he distinctly mentions the sources of his information and of his illustrations. While on this subject we may mention that the only radical cure for entropium as met with in this country is that practised by Sir Wm. Wilde; we would again also remark that the plan of treating staphyloma by

abscission, attributed by Mr. Power and others to Mr. Critchett, was described and performed by Sir Wm. Wilde very many years ago. We lately saw a person on whom Sir William had performed this operation upwards of twenty years ago!

We were somewhat surprised to find so little information on the treatment of orbital aneurism, a subject of such great importance to us all. We believe the carotid artery has been tied for orbital aneurism upwards of thirty times, and yet on looking to Mr. Power for the latest information on, and the statistics of the subject, we are briefly referred to the eleven cases enumerated by Demarquay in his *Traité des Tumeurs de l'Orbite*, of which we gave an account in the number of this journal for November, 1860. We cannot but regret that the large space given in a previous chapter to the comparatively useless detailed measurements of the orbits of forty-eight skulls had been given to such a practical matter as ligature of the carotid.

The plan of treatment of gonorrheal opthalmia "now generally practised" consists, according to Mr. Power, in supporting and stimulating the patient by "half-a-pound of rump-steak twice a day, with a pint of beer," the removal by an attendant of all collection of matter, and the prevention of "its accumulation by the injection from an india-rubber bottle of either pure cold water or a solution of alum, containing two grains to the ounce, regularly every quarter or every half hour throughout the day, dropping into the eye after each ablution a drop of nitrate of silver of the strength of two grains to the ounce; or, as Mr. Collis recommends, one quarter of a grain to the ounce." No allusion is made to the compress bandage—a valuable aid in the treatment of this affection; neither are we informed what should be done in cases of extensive chemosis. Now, a large number of these cases do not apply for medical assistance until the lids are swollen and œdematous, and the conjunctiva chemosed, bulging up over the cornea and momentarily threatening its vitality; in such a case we never hesitate to freely open the bag of the conjunctiva, and thus relieve the congestion, free the strangulated cornea, and prevent its perishing and bursting.

Mr. Power would appear to be an advocate for Mr. Hancock's condemned operation of dividing the ciliary muscle in anterior and posterior sclero-choroiditis, in suppurative keratitis, staphyloma of the cornea, sympathetic ophthalmia, and seems to prefer it to irridectomy in the treatment of glaucoma.

Here and there the phraseology is somewhat peculiar; thus at page 39 the author speaks of "the distension and *pullulation* of the outer parts of the globe which are associated with" myopia; at page 22, tarsorhaphy is said to "consist in *refreshing* the edges of the lid (? lids) at the outer canthus, and bringing them together with sutures;" at page 522, in alluding to a case of flap extraction, it is stated that "on making the section *the knife carried off the lens.*" At page 596 *diverging* strabismus is said to occur frequently in hypermetropia; this we take to be a clerical error for *converging*. There are some other clerical and typographical errors throughout the work which will, we have no doubt, disappear in a second edition.

Taken as a whole, however, the work is an excellent exposition of the present state of knowledge, and may be regarded as one of the best treatises on diseases of the eye in general in the English language, for it enters into the consideration of nearly every affection, whether seen by the unassisted eye or by means of the ophthalmoscope, and embodies all the latest pathological observations. We would recommend the work specially to those who may not possess Wecker's splendid work *Maladies des Yeux*, or who may not have access to the German or French literature. From the nature of the work, and its great range of subjects, it is almost impossible to select suitable extracts. We append a couple, which will illustrate the character of the book. At page 414 the author remarks:—

"The pathology of glaucoma is still obscure. v. Gräfe, with most modern ophthalmogists, regards the forms of the disease which have just been described under the terms of acute and chronic glaucoma as the true types, and consequently maintains that it is essentially an inflammatory affection, the choroid being the membrane primarily and chiefly affected. On the other hand, M. Donders, with whom M. Wecker appears to coincide, considers that simple glaucoma, or that form in which excavation of the optic nerve with preservation of the transparency of the media occurs, is really the type, to which inflammation may or may not be superadded, and that it essentially consists in a neurosis or functional disturbance of the secretory nerves of the eye, in many instances maintained and intensified by tension and traction exerted upon the iris. He remarks that, in simple glaucoma (the last of the above-described forms), the eye acquires a remarkable degree of hardness, which is clearly due to hypersecretion of the humours of the eye, and which is yet unaccompanied by the least trace of inflammation. He believes that these

secretions are under the influence of the nervous system, and supports his view by reference to the effects of section of the fifth pair of nerves, which induces an excessive softness of the globe, and is accompanied by a singular diminution of the secretion of tears. On the contrary, when from any cause the secretory nerves are irritated, the secretion of the vitreous is augmented, and the lens and iris are pushed forwards, but the traction and tension of the latter again react as irritants upon the secretory nerves, causing a further increase of secretion and of intra-ocular tension. It is to be remarked, however, that though diminution of the intra-ocular pressure is well-marked after section of the fifth, it is by no means constant after pathological changes, involving destruction of the nerve.

"Another view of the pathology of glaucoma, to the effect that it is primarily associated with *rigidity of the sclerotic*, has been advanced and ably supported by M. Stellwag v. Carion, who considers that the increased tension of the globe observed in some cases of glaucoma, where no inflammatory processes have been or can be shown to be present, is opposed to v. Gräfe's view, and that M. Donders' theory is supported by no matters of fact, but rests only on analogy.

"M. Stellwag v. Carion[a] observes that the hardness of the globe had long ago been pointed out by Dr. Mackenzie as a fundamental feature of the disease, but that up to the present time no clear perception of its nosological significance has been obtained, whilst it has been erroneously considered as an expression of, and synonymous with, increased intra-ocular pressure. In point of fact, however, the perceptible resistance of the globe to pressure is no simple phenomenon, but results on the one hand from the amount of intra-ocular pressure, and on the other from the degree of elastic extensibility (resilience) possessed by the ocular capsule. It is impossible to consider the two factors separately. It can only be said that any augmentation of the pressure in the interior of the eye increases the tension of the capsule, and consequently the perceptible resistence; but at the same time the eye must, with unchanged internal pressure, appear so much the harder in proportion as the sclerotica is less yielding and extensile. Similar conditions are present in the case of the hardness of the pulse in the larger arteries, the hardness being always greater in stiff atheromatous arteries than in normal arteries, especially in those of childhood.

"Now, rigidity of the sclerotic and consequent unusual hardness of the eye (so v. Carion continues to argue) is not infrequent as a natural condition, perhaps inherited and congenital, and tending to increase with age, but still more frequently as an acquired condition due to senile changes and atheromatous degenerations. That in such cases the hardness does not

[a] Lehrbuch der Prakt., Augenheilkunde, p. 270. 1867.

proceed from any increase of the intra-ocular pressure, is abundantly shown by the circumstance that, as a rule, neither subjective nor objective functional disturbances are present, and that both eyes may even permanently retain their full integrity of function.

"The physical changes occurring in the sclerotic, and occasioning its rigidity, have not been very accurately ascertained; but Coccius has observed the presence of fatty, and Donders of calcareous degeneration. But if now the increase in hardness of the globe be not necessarily combined with an increase of the intra-ocular pressure, but is attributable to other causes, it is difficult to admit the latter as constituting an integral constituent of the glaucomatous process; rather it would appear that the increased resistence of the bulb in simple glaucoma, and in the remissions of the inflammatory form is to be referred purely to the rigidity of the capsule of the bulb; and the circumstance that the resistance which the globe offers to external pressure in indubitable cases of glaucomatous affections is frequently not greater than that which is met with in perfectly sound eyes, with rigid capsule, is a strong argument in favour of this view. The constant occurrence of excavation of the optic disk in glaucoma cannot be considered as a valid proof of the temporary or permanent augmentation of the intra-ocular pressure, since this may well result from diminution of the resistance of the cribriform layer, which may, *per se*, without any alteration of the normal pressure, occasion the excavation; and such diminution of its resistant power may be induced by simple neuritis, atrophy, or inflammatory change, such as is frequently observed to accompany the acute glaucomatous processes.

"In conclusion, M. Stellwag observes that, if an increase in the amount of intra-ocular pressure ceases to be an essential element in the production of glaucoma, the rigidity of the sclerotic capsule alone remains, which accounts for the rare occurrence of glaucoma in young subjects, and likewise for the curative results of the modes of operation found effective in obtaining relief; since in these the sclerotic is divided through a considerable arc, and the wound is closed by a yielding cicatrix, which in some degree supplies the absence of elasticity of the ocular capsule itself. He then goes on to show how the various changes above noted may result from the circulatory disturbances induced by the rigidity of the sclerotica."

After referring to the causes and mode of origin of panopthalmitis, Mr. Power continues:—

"Under whichsoever of these processes the inflammation, leading to general suppuration of the globe, may set in, the patient usually experiences intense pain in the eye. which rapidly extends to the temple, brow, or cheek; the lids become swollen, red, and œdematous; the eye-ball bulges forward; the conjunctiva becomes chemosed; the subconjunctival

vessels are large and numerous, giving the eye a dull appearance, and a circum-corneal zone becomes more or less marked; the surface of the cornea loses its polish, and it presents a nebulous appearance throughout its whole thickness; the iris is pressed forwards, diminishing the depth of the anterior chamber; its markings are confused, and the pupil is either small or, more generally, widely dilated. In the latter case, especially after severe blows, blood may be frequently discovered in the vitreous, which, even without this, is so hazy as to render an examination of the fundus difficult. The tension of the globe increases; it becomes exquisitely tender; and violent throbbing is experienced, coincidently with which the formation of pus takes place in its interior.

"Where the primary injury has been a blow, the sight is usually immediately lost; but when it is the consequence of inflammation, extending from the outer tunics, vision is only gradually lost by intra-ocular pressure, and sparks, flashes, and other subjective symptoms of irritation of the optic nerve, are complained of.

"In the cases which occur after an attack of the epidemic cerebro-spinal meningitis, as well as in the metastatic form seen in pyæmic conditions of the system, both eyes are almost always simultaneously affected. This occurred in nine out of eleven cases observed by Jacobi, and in every instance the eye or eyes affected were completely lost.

"In traumatic cases, the disease is, of course, limited to the one that has received the injury, but the other is extremely likely to suffer from sympathetic ophthalmia.

"The pathological changes which occur in these cases have been the subject of much discussion and of many inquiries, of which those of Schweigger,[a] Ritter,[b] Schiess,[c] and Weber,[d] are most deserving of perusal, and constitute the greater part of what is at present known respecting the microscopic changes which take place in the several humours and tunics in the progress of acute inflammation.

"C. Schweigger observes that, in advanced cases of choroiditis, where the whole of the choroidal stroma is infiltrated with pus, the exact starting-point of the suppurative process is very obscure; but in cases of more recent date, the formation of small collections of pus may not unfrequently be found to be limited to the internal vascular layer of the choroidal *stroma*, where they may be easily overlooked on account of the strong adhesion of this layer to the true capillary layer of the choroid, or chorio-capillaris. Hence we have here an exemplification of the general fact that the earliest occurrence of suppurative changes take place by the side of vessels; and M. Schweigger has assured himself that the pus cells proceed from

[a] Archiv. f. Ophth., Bd. v. Heft. ii. p. 216.
[b] Idem, Bd. viii. p. 18.
[c] Idem, Bd. ix. p. 22.
[d] Virchow's Archiv., Bd. xix. p. 367.

alterations and proliferations of the corpuscles of the conective tissue with which the vessels are so abundantly accompanied. Very frequently, however, purulent collections take place between the choroid and the retina, accumulating till these membranes are completely detached from one another. The retina then becomes coarctate, or extends like a cord, column, or funnel-shaped body, from the optic disk towards the surface of the lens, against which it may even be compressed, as a grey semi-transparent pultaceous mass, by the rupture of its continuity with the papilla. The lens is pushed forwards by the gradual increase in the purulent mass, and the anterior chamber is reduced to extremely small dimensions. Besides the alterations which occur in the connective-tissue corpuscles, attended by the formation of pus, considerable changes take place both in the stellate pigmentary cells of the choroidal stroma, and to a less degree in the hexagonal cells of the internal pigmentary layer, which in some parts lose their pigment, and in others contain pigment of an unnaturally dark hue. Here and there, in both portions of the membrane, cells may be seen, which appear to be passing into a condition of fatty degeneration. M. Schweigger believes that the development of pus cells may take place in the substance of the retina, and, by gradually breaking down its elements and diffusing themselves into the vitreous, may lead to atrophy and pigmentation of that tunic. As regards the vitreous, he has observed in acute cases an active process of cell-development in its structure, some of the new cells being large, branched, anastomosing with one another, and nucleated; others smaller, and presenting intermediate forms between these and pus corpuscles. They sometimes develope pigment in their anterior. As regards the lens, he only observes that its opacity commences in the outer layers.

" Dr. Ritter's experiments, made with a view of determining the progress of violent general inflammation of the eye, and the origin of the pus, were performed on rabbits. He introduced a needle, and pressed the lens against the choroid with considerable violence. The results which he obtained may be best understood by describing successively the changes observed in the several tissues.

" As regards the vitreous, it must be remembered, Ritter holds, that in the normal condition no cellular structures are to be found distributed through its tissue. Cells occasionally appear in inflammation, but are then, he believes, derived from the choroid. In his opinion, simple injury to the vitreous, even though extensive, never leads to the formation of pus within it, nor has he ever observed any extravasation of blood. The first appearance of pus always appeared just within or without the lower part of the hyaloid membrane, and, gradually increasing in amount, pressed towards the centre of the vitreous, the upper part of which usually remained clear to the later stages. He considers that although, as Schweigger had previously stated, pus cells may be found alike in the

vitreous, retina, and choroid, they can only be developed in the latter membrane, since in the two former tissues the anatomical elements fail, out of which pus corpuscles can originate. The real origin of the pus cells is to be looked for in the unpigmented stroma cells of the choroid, and in the nuclei of the muscular-fibre investment of the vessels, which enlarge and divide. The branched pigment-holding cells of the stroma, on the other hand, long remain quite unaltered, though they ultimately break down, and leave only little clumps of pigment. As soon as inflammation is established in the choroid, its vessels increase in diameter, and subsequently the connective-tissue cells enlarge, and, together with the nuclei, increase in number, exerting so much pressure upon the vessels as to effect their obliteration. A fibrinous exudation is poured forth, diffusing itself equally through all the intra-ocular tunics. In the meanwhile, the inner layer of pigment cells has undergone various changes. Many lose their hexagonal form, become enlarged, pouched, and pressed together, so that the clear lines of separation normally present are lost; the pigment becomes distributed in a thin layer beneath the cell-membrane, whilst the central and larger part of the cell remains clear, and often contains two nuclei. Some of these cells, breaking down, permit the passage of the pus cells, hitherto lying on their outer side, into the space between the choroid and retina, where they form thin layers.

"The retina still opposes a certain resistance to their entrance into the vitreous, but soon they may be seen lying between the elements of the retina, which are separated from one another, and broken down, and at length they gain admission by one or many points into the vitreous. Fragments of the membranæ limitantes, nerve cells, radial fibres, and elements of the bacillar layer may then be found near the centre of the globe; but, even at this stage, isolated patches of the retina may be seen presenting its natural structure. In some instances, it would appear that the pus may enter the vitreous by another path, viz., through the separation of the retina at the ora serrata from the zonula of Zinn, but this does not occur to any great extent.

"The vitreous, in most instances, becomes at an early period so reduced in consistence as to be almost or completely fluid, whilst its transparency is greatly impaired by the presence of minute granules, and by degenerated epithelial cells. It is ultimately absorbed, and the whole posterior chamber of the eye becomes filled with pus, with which event this stage of the process terminates.

"The part played by the iris is important. Often at first it is only slightly affected, the cells slowly undergoing changes analogous to those already described in the choroid. Sometimes, however, the inflammatory symptoms are more active, pus, in the formation of which the muscular-fibre cells of the vessels may be distinctly seen to take a part, being

developed, which, falling the bottom of the anterior chamber, constitutes an hypopyon, or, as more frequently occurs in rabbits, on account of the viscidity of the liquor puris, forms a consistent mass projecting into the anterior chamber. The epithelial cells of the uvea, and the pigment cells of the iris, are very resistent of change.

" The changes occurring in the retina during the suppurative process are that the radial fibres enlarge, become granular, and the nuclei multiply. The nerve cells also become granular, and their contents frequently undergo fatty degeneration. In the external granular layer, hyaline spherical bodies may occasionally be seen, often three times as large as pus corpuscles. They resemble mucus corpuscles, possess no investing membrane, but strongly refract light. M. Ritter considers that they are formed by the contents of the bacilli, pressed out by the pressure of the surrounding pus cells. The inner portion of the bacilli cannot in general be traced.

" The capsule of the lens, and the epithelial cells of the anterior capsule, never exhibit any pathological change, even to the very last. For some time the separated portions of the lens remain clear, but subsequently they become cloudy, the outer layers of fibres being detached from one another, irregularly crumpled and infiltrated with pus, whilst the more deeply-seated fibres are scarcely altered, being only lightly punctuated with irregular swellings on their edges. Remains of the lens are always found, even in atrophic eyes.

" As regards the outer tunics, the sclerotic always remains unchanged, but the conjunctiva becomes vascular and chemosed. A well-marked circum-corneal zone is formed, and the cornea is covered with vessels, which Ritter found on the twenty-fifth day had nearly reached the centre, and the corneal tissue is much softened ; the nuclei are enlarged but not multiplied. When these processes have run their course, the eye begins to shrink, and its complete atrophy is the final result. As regards the time occupied in these processes, M. Ritter's examination showed that from the sixth to the tenth day pus had already been formed in considerable quantity, breaking down the retina, and penetrating into the vitreous. On the twelfth day the pus occupied about one-half of the posterior part of the bulb, and by the sixteenth to the twenty-fifth day, the whole of the vitreous was infiltrated. The globe, however, had at the same time lost but little of its tension. The later stages were not noted, all the eyes being removed for examination at this period.

" I venture to give one more account of the pathological changes observed in panophthalmitis—that of M. Schiess, who also experimented on rabbits, and established general inflammation by dividing the middle of the cornea in several directions with scissors. The iris soon protruded through the wound, the centre of which formed a kind of funnel-shaped cavity, occupied by a plug of coherent pus that constantly underwent

renewal. The whole eye assumed a conical form, and diminished in size and firmness. The conjunctiva became œdematous, and there was considerable purulent discharge. In one case, where the eye was removed on the twenty-ninth day after the injury had been inflicted, and was examined after eighteen days' immersion in chromic acid, it appeared like a utricle filled with pus, the anterior swollen edges of which consisted of the iris fused with the altered corneal tissue, and infiltrated with pigment. The central opening formed a channel by which the pus, so long as it was developed in the interior of the globe, could be discharged—until, in fact, complete atrophy or phthisis bulbi took place, no traces of the lens nor of the vitreous, except near the optic entrance, could be discerned. The sclerotic appeared somewhat thickened, and in consequence of a condensation of the fibres of the lamina fusca, was intimately attached to the choroid. Posteriorly, the limits of the two were clearly defined; but in the neighbourhood of the ciliary processes they appeared to be fused together. At this part some increase in the connective-tissue corpuscles of the sclerotic was noticed, especially near the corneal margin. The same was observed in the superjacent connective tissue, where a little pus had formed. The proper stroma of the choroid was decidedly thickened, the interstices were filled with nuclei or small cells, without any very definite cell-wall, and there were numerous fusiform bodies, each of which appeared to contain several nuclei. M. Schiess does not entirely agree with Ritter in thinking that the pus cells originate from choroidal stroma cells destitute of pigment, but their endogenous formation from other cells is perfectly obvious. The number of pus cells constantly increased towards the inner surface of the choroid. The condition of the blood-vessels was very remarkable, as they appeared to have atrophied in consequence of pressure made upon them by the process of cell development. M. Schiess appears to think that the real seat of origin of the suppurative process is in the connective tissue surrounding the vessels, and is of opinion that Ritter's preliminary stage of dilatation of the blood-vessels is doubtful. The focus or chief seat of the purulent deposit was in the chorio-capillaris, nearly one-half of the entire increase in thickness of the membrane being due to the changes occurring in this layer, the cell proliferation being in truth enormous. The internal layer of pigment cells retained for the most part the regularity of their arrangement, though in some places they appeared to have given way under the pressure of the new formations. Every part of the inner portion of the choroid was thoroughly infiltrated with pus cells, but no evidence could be obtained of the endogenous formation of pus cells from the pigment cells. In no instance was the basement membrane of the choroid recognizable. The retina presented different appearances according to the local intensity of the production of pus cells, and of the serous exudation. Although nowhere healthy, its elements here and there preserved their normal relations to other parts,

and especially in those cases where it was still covered by any remains of the vitreous. An especially active proliferation process appeared to have taken place near the entrance of the optic nerve, which was distinctly excavated. Around the optic papilla the retinal fibres appeared torn, and were directed straight forwards in bundles, which were thoroughly impregnated with pus. The whole retinal tissue appeared dull and hazy, and in parts was converted into fibrous or granular heaps, lying on the choroid. In other parts the tissue was infiltrated with serum, and radiating fibres were seen, together with large pale nuclei, a few ganglionic cells, and elements of the granular layer. The radial fibres generally appeared swollen, broad, and with fibrous structure. The outer limitation against the choroid was more distinct than the inner against the vitreous, in which last, fragments of the retina lay loose; or perhaps, more correctly speaking, the place of the vitreous was occupied by a chaotic mass of retinal tissue, disintegrated choroid, and pus cells. He saw no remains of the membranæ limitantes."

The work is profusely illustrated with twelve plates containing seventy-two chromolithographs, and numerous woodcuts scattered through the text. The coloured illustrations, though not equal to Dalrymple's or Sichel's splendid plates, are pretty fair representations, and reflect great credit on Mr. Power as an artist. Figure 32 is a good representation of suppurative keratitis with pus in the layers of the cornea and in the anterior chamber. We would again take exception to and reverse the nomenclature of Mr. Power and others; we maintain with Sir Wm. Wilde that pus in the anterior chamber should be called onyx, and pus in the cornea hypopyon. Eighteen coloured figures are illustrative of diseases seen by means of the ophthalmoscope.

On the Action of Medicines in the System. By Frederick William Headland, M.D., &c. 8vo. Fourth Edition, Revised and Enlarged. London: Churchill and Sons. 1867.

Études sur Les Médications Arsenicale et Antimoniale et sur les Maladies du Cœur. Par Le Docteur Lucien Papillaud (Henri Almès). Paris, 8vo. J. B. Baillière et Fils. 1867.

As regards the action of medicines, Dr. Headland has long been one of the men of the time; his work needs no introduction from us,

and therefore we shall not occupy the time of our readers with discussing its merits, which we all admit; but we shall offer some remarks on this, the fourth, edition, from the preface of which we make the following extract:—

"A considerable number of fresh observations and discoveries, some of them of considerable importance, and representing the labours of therapeutists of all nations during the last seven years, have been noticed in their proper place in the present volume.

"A part of the scheme of this work which has been much criticised will be established by the confirmation of a discovery which has been lately announced, that quinine, or a substance resembling it, is natural to the blood.

"The molecular theory of the action of nerve medicines, put forward with some hesitation by the author of the essay, has also received much corroboration from recent researches."

Turning over to page 150, we find something worthy of special note in reply to the question, as to that class of Hæmatics of which quinine is the type:—"*Are they naturally present in the blood, or is there in healthy blood any substances which resemble them?*" It may be stated, with reference to this question, that since the appearance of Dr. Headland's original essay in 1852, various critics found fault with him, not so much for his having placed vegetable tonics in the class of blood medicines; but for his confidence in having grouped them in the first division, Restoratives, rather than in the second. He admits that these criticisms were made "perhaps not altogether without reason;" and he explains the opinion so treated somewhat after this fashion:—Taking quinine as the type, he observed that its action was permanent, and that it had no direct influence on the nerves. Having also noted two facts: 1. That the quinine taken was not wholly eliminated from the body; and 2. That it had no toxic action of its own on the blood; he decided to place that medicine, and the whole group at the head of which it stands, in the division of Restoratives, rather than among the Catalytics. However, what he terms "the most important part of the demonstration," was still wanting. In order that the Tonic should act as a Restorative, it must supply the place of something normally present in the healthy blood. Until recently there was no proof of the presence in the natural blood of man or animals of any substance similar to quinine. But Dr. Dupré and Dr. Bence Jones, in experimenting as to the feasibility of discovering quinine, by the

"fluorescent" test of Stokes, in the blood of a guinea-pig (to which it had been administered), found not only that they could so discover it, but they also obtained the same reactions with the blood of another guinea-pig which had not taken quinine at all. From this it follows, in Dr. Headland's opinion, that quinine, or some substance chemically identical with it, is contained naturally in the blood and tissues of this animal; and, he continues, "as whatever is present in the blood of rodents must be present in that of man, quinine, or some substance chemically resembling it, occurs in human blood." He then proceeds to remark that the matter is of very great importance to his argument, and "in order that there may be no mistake about it" he subjoins the following account of this "remarkable discovery," as given by Dr. Jones himself in a lecture at the Royal Institution, in April, 1866:—

"No imagination could have anticipated that the line of research into the rate of passage of substances into and out of the textures would lead to the supposition that man and all animals possess, in every part of the body, the most characteristic peculiarity of the bark of the cinchona trees of Peru. After determining the rate of passage of lithia and other mineral matters into and out of the body, Dr. Dupré and I proceeded to endeavour to trace the rate of passage of quinine into and out of the textures of animals. We chose quinine because of that splendid test which led Professor Stokes to the discovery of the change of refrangibility of light.

"A guinea-pig was given quinine, and for comparison another guinea-pig was killed at the same time, having had no quinine. In the pig that had taken quinine, each organ was heated in a water-bath, with very dilute sulphuric acid. This extraction was repeated over and over again. The acid extracts were mixed and filtered after cooling, neutralized with caustic soda, and repeatedly shaken up with their own bulk of ether. The residue left after evaporation of the ether, was taken up by dilute sulphuric acid, filtered and tested for fluorescence. The pig that had taken no quinine had each organ treated in a precisely similar way. To our great disappointment, at first, we found not only had the pig that had taken the quinine a fluorescent substance in the textures, but that an almost exactly similar substance was extracted from the organs of the pig that had taken no quinine. Every texture was examined, and in every one this fluorescent substance occurred. We then endeavoured in every possible way to find a means of separating the natural from the induced fluorescence. And as every method failed, and we were compelled to recognize the close similarity of the substance that exists in the textures to quinine itself, we for a time dropped the original

inquiry, and proceeded to a more complete investigation of the natural fluorescent substance in animals.

"Having obtained solutions, we were able to compare them with solutions of quinine in their actions on the spectrum. And first, the solution of the natural substance begins to fluoresce a little before the solution of quinine; but on carrying it on through the spectrum it ends where quinine ends. The fluorescent light of the natural substance is a little more greenish than the fluorescent light of quinine. If a quartz cell containing this fluid is interposed between the source of light and a solution of quinine, no fluorescence takes place in the quinine; and if quinine is interposed between the light and this natural solution, scarcely any fluorescence is observed in it. When a solution of salt is added to the naturally fluorescing substance, it is almost entirely destroyed, as happens with quinine. If the natural solution is boiled with permanganate of potass, it does not lose its fluorescence, nor does quinine; but when permanganate with excess of alkali acts upon this substance or upon quinine, the fluorescent substance is entirely oxidized. Hence this substance, by the mode of its extraction, and by its remarkable action on light, is very closely related to quinine; and this led us to apply the chemical tests for quinine to this natural fluorescent substance, after extraction from the body. The different tests for alkaloids like quinine, as morphia, strychnine, veratrine, atropine, are as follows:—First, quinine gives a precipitate with iodine in iodide of potassium. Secondly, iodide of mercury in iodide of potassium also gives a precipitate. Thirdly, phospho-molybdic acid also gives a precipitate. Fourthly, bichloride of platinum gives a precipitate. Lastly, terchloride of gold causes a precipitate, and this precipitate is soluble in alcohol. Now each and all these different reactions are obtained with these same reagents acting on the fluorescent substance that is extracted from animals. So that here again we have chemical proof that this substance is an alkaloid, and that it is closely related to quinine. We have named it Animal Quinoidine because we have not as yet been able to crystallize it nor to obtain enough for analysis. . . . . Our so-called animal quinoidine is descended from albumen, and its ultimate progeny are corbonate of ammonia and water, out of which substances the cinchona-tree, under favourable circumstances, is able to build up quinine,—

$$C_{40}H_{24}N_2O_4$$

"From the large number of carbon atoms in quinine, it may be regarded as one of the early substances produced in the downward passage of albumen, and from this we shall very probably find the key to the question how quinine acts in the body."

Although forming part of the original essay, we may, even now, direct our readers to page 285, where some most sensible remarks

are made on the use of tea and coffee as common articles of diet: "They are, in fact, almost indispensable to the daily existence of civilized man, with whom a continual energy of mind is a necessary of life."

On page 338 Dr. Headland thus remarks on a recently agitated question; and we fully endorse his views:—

"Because diarrhœa is under the influence of purgative medicines, it has been proposed to rely upon them in the treatment of Asiatic Cholera. Dr. George Johnson has thus used Castor-oil in the treatment of this terrible disorder. Calomel in purgative doses, Croton-oil, and other Cathartics, have been given by others. In 1866 this principle of treatment was fairly and patiently tried by a large number of physicians, but with no better success than other pretended plans of cure. It is no doubt true that the colliquative purging of cholera depends upon the existence of something which has to be eliminated from the blood; but this need of elimination is not the essence of the disease, any more than it is of Typhoid or Gastric Fever. Castor-oil will not cure these disorders, nor will it cure Cholera. No purgative medicine can have power to arrest this wholesale transformation of the blood into 'rice-water' discharges. The contrary is the case with diarrhœa of the ordinary kind. It only needs the setting-up of a healthy eliminative process. It is this erroneous parallel between Cholera and Diarrhœa which has led physicians into this error. Cholera is a disease *per se*, and only accidentally connected with diarrhœa. I am inclined to think that the 'premonitory diarrhœa' so much spoken of is in many cases imaginary, in others greatly exaggerated—that the cases of this affection which are cured are mere ordinary diarrhœas occurring during an epidemic of Cholera. Cholera is an epidemic disorder, produced by a specific poison which may be carried in water, and is contagious, probably, through the medium of water thus contaminated. It is a disorder which runs a certain course, and goes through several stages if the patient lives long enough. It produces a remarkable depression of the nervous system, sometimes altogether disconnected from the bowel affection. This, and the arrested oxidation of the blood, and the suppression of urine, are symptoms quite as dangerous to the patient as the alvine flux. If we cannot cure Cholera by eliminatives, neither, as it seems to me, can we hope to cure it by astringents. It is time that we had left off talking of cures for Cholera. When a patient has taken Scarlatina or Small-pox, we know that the disease must run its course, and that we cannot arrest it. It is the same with Cholera. We can only treat symptoms, we cannot check the disease, which must go on to the end like so many other morbid fermentations."

Dr. Headland expresses no opinion as to the merits of Dr. Dobell's widely advertised Pancreatic Emulsion. He mentions it, in the section on Cod-liver Oil, in the following terms (page 362):—

"Dr. S. Dobell has employed beef-fat and lard-oil made into an emulsion with the pancreatic juice of the pig, as substitutes for cod-oil. He considers his pancreatic emulsion superior to ol. morrhuæ in most phthisical cases."

On another question, which went the round of the medical journals some time ago, we make no apology for giving Dr. Headland's *ipsissima verba* from page 400:—

"An account of the therapeutical actions of arsenic would be incomplete if it did not include some notice of the extraordinary fact which now appears to be established, of the tolerance of large doses of this drug by persons who have become by degrees habituated to it. We are familiar with this phenomenon of tolerance in the case of nerve-medicines; with blood-medicines it is so rare that we should decline to believe in it until convinced by overwhelming evidence. I have long been aware of the fact that larger and larger doses of this poisonous mineral are borne with impunity by patients who have been for some time under treatment for cutaneous and convulsive affections. A case has been brought under my notice by Mr. Milton, in which as much as 400 drops of Liquor Arsenicalis were taken in a week without injurious consequences. Facts like these prepare us to accept, without dispute, the stories of Arsenic-drinkers and Arsenic-eaters in this and other countries, which for so long a time were scouted as apocryphal by scientific men. Dr. Davy and Mr. Church tell us that water containing an appreciable proportion of arsenic (·064 grains in a gallon) is used with perfect impunity for drinking purposes by the inhabitants of Whitbeck, in Cumberland. (*Edin. Phil. Journal*, and *Lancet*, July, 1863.) Long ago, an Austrian physician, Von Tschudi, assured us that arsenic was eaten in considerable quantities by the peasants in the mining districts of Lower Styria, and was in favour among them as a means of improving their complexions. The statement, reiterated in various popular works, was alluded to as incredible by the great majority of writers on toxicology, and on the action of medicines. Dr. Taylor (*On Poisons*) pronounced the story 'absurd.' Dr. Lorenz of Salzburg, has, however, corroborated the tale of Von Tschudi, in forwarding to Mr. Heisch, of London, in 1860, some positive evidence obtained from the directors of the arsenic works in Styria. Dr. Maclagan, of Edinburgh, and Dr. Rutter, determined to set the question at rest, visited Styria, in the spring of 1865. They were enabled to verify the fact that large doses of arsenic (arsenious acid and the sulphuret) were taken habitually and with impunity by natives of

that country. One man was observed to take about five grains, another six grains, of white arsenic at a dose. (*Edin. Med. Journal*, April, 1865.) Dr. Hardy doubts whether more than a small portion of the arsenic thus taken in the solid form can be absorbed into the blood. But one grain of white arsenic is a poison to most men. So that, until these positive statements, made in several quarters, can be satisfactorily accounted for or confuted, this phenomenon of the tolerance of arsenic must be admitted as a fact."

We must say that we coincide entirely in Dr. Headland's views respecting the use of alcohol. Much of what appears on this subject in the present edition appeared in the original essay; but here we find some alterations chiefly in the way of addition. He stands up for the soundness of the principle of the view taken by that late eminent physician, Dr. Todd; admitting, as we freely admit, that he may have pushed somewhat too far his advocacy of alcoholic stimulants in acute diseases. Nevertheless, Dr. Headland considers that Dr. Todd's error, "if error it was," was *in the right direction*, "as there is far less danger of doing mischief by stimulants than by depressants." On the question—is alcohol a calorifacient agent, or is it not?—Dr. Headland's opinion is not definite! It "lies mid-way" between the opposite conclusions of Liebig, Vierordt, Prout, Snow, Smith, Lallemand, Perrin, and Duroy; all of which he mentions and discusses. "Alcohol, taken in moderate quantities, is burnt in the blood. When in excessive amount, a part of it remains undecomposed in the blood, and is excreted unchanged." If space permitted, we should enter on a discussion of the best and most important part of the whole book; the section on opium, extending from pages 417 to 431. We specially commend this section to the notice of our readers. It not only treats of opium in its every phase, as it concerns man, but also as it concerns some of the lower animals. It seems that large quantities are required to poison rabbits and other rodents; while apes are completely poison-proof so far as opium is concerned. Flandin administered to one 500 grains without effect; and Dr. Headland accounts for this immunity by the fact of its rapid elimination in the urine. He considers the pernicious practice of opium-eating much increased in England, particularly among the operatives in the manufacturing districts; and he also mentions its existence among the field labourers in Lincolnshire; observing, at the same time, that "Government has not interfered, and will not interfere to stop this. Most wisely has the sale of arsenic been

restricted by law. But any one may buy opium. The recorded deaths from suicide and murder, which result from its unrestricted sale, are increasing in a frightful ratio." In these days of sanitary reform, and hygiene, and loud talk about good water, and pure air, we heartily wish that some influential political agitator would turn his abilities to real usefulness, and teach our senators wisdom in this matter, which so loudly and justly calls for immediate reform.

Papillaud.—In the preface of this tractate, which is dated 18th July, 1867, from Saujon (Charente-Inférieure), M. Papillaud very sensibly remarks that while priority of invention is a much cherished distinction, yet that it has serious inconveniences for the discovery as well as for the inventor. It must encounter the hostility of some, and the indifference of others; while a new idea has occasionally been looked on as "une personnalité à l'addresse de ceux qui ne l'ont pas eue." On these accounts, M. Papillaud disclaims priority as regards this treatise, but avows his present production to be the result of solid work of his own, nevertheless. The first portion of the book treats "De Medication Arsenicale;" and may be referred to as containing all that has been advanced and need be known about it. The discoveries of others are carefully marked, and due credit is given to every labourer in the arsenical vineyard. It opens with an abstract of the opinions of Boudin in 1840; and thus speaks of the origin of Trousseau's arsenical cigarettes:—

"Pendant cette année, 1841, le professeur Trousseau essaya l'arsenic contre la phthisie et le catarrhe pulmonaire. Il le donna à l'intérieur et l'employa aussi en cigarettes pour en faire respirer la vapeur. Ces indications étaient fondées sur un passage de Dioscoride, qui préconise l'arsenic pour les malades qui ont du pus dans la poitrine et pour les asthmatiques, indiquant comme mode d'administration interne son mélange avec du miel et des substances résineuses. Son alliance à des vapeurs résineuses est également mentionnée par Dioscoride, et le professeur Trousseau se conforma à ces indications en faisant imprégner de baume de Tolu ses cigarettes arsenicales. Cette medication procura du soulagement à ceux qui y furent soumis, mais ne donna pas de guérisons."

Full mention is made of almost every name known to the profession in connexion with this subject; and this part of the treatise winds up with his own views, a portion of which appear in the following extract from page 19, and we insert it, not only

for this reason, but also because of its reference to the Styrian arsenic eaters, of which we have spoken above when discussing Dr. Headland's work:—

"Depuis la publication du mémoire du docteur Tschudi sur l'emploi vulgaire de l'arsenic comme médicament hygiénique parmi les populations de la Styrie, du Tyrol et de quelques autres contrées de l'Allemagne, une certaine curiosité s'est emparée du public à l'égard de cette substance. On a voulu en France se donner, comme les Allemands de la basse Autriche, la fraîcheur du coloris et l'éclat des yeux au moyen des préparations arsenicales, et non-seulement ce médicament a refait le teint mais encore les forces et l'embonpoint, il a régularisé la respiration et la circulation, répandu le bien-être et l'harmonie dans l'organisme, en un mot il est devenu un reconstituant. C'est sous ce dernier rapport qu'il a été considéré, étudié e expérimenté dans ces dernières années par les docteurs Isnard et Wahu, dont les récents travaux ont définitivement tiré l'arsenic de l'ornière de l'anti-périodicité dans laquelle il était resté embourbé pendant si longtemps. Aujourd'hui toute dissertation sur sa vertu fébrifuge serait une redite; sur ce point la discussion est épuisée, la lumière est faite et le débat est clos. La propriété de guérir la fiévre intermittente est une des moindres qualités de l'arsenic; c'est comme médicament d'un usage presque général et s'appliquant au plus grand nombre des maladies qu'il doit être considéré maintenant. En effet, sa qualité de reconstituant ne pose-t-elle pas son indication en regard de presque tous les cas pathologiques? L'action de la maladie n'est-elle pas une action déconstituante et le rôle du remède n'est-il pas de refaire ce que la maladie a défait ou de maintenir ce qu'elle tend a désorganiser?

"La medication arsenicale est indiqée, selon nous, dans tous les cas où il est opportun de relever les forces et de régulariser les fonctions quelle que soit d'ailleurs la nature de la maladie qui amené cette indication. L'action réparatrice de l'arsenic trouve aussi bien sa place à la suite de l'affaiblissement causé par une pneumonie qu'à la suite de l'anémie produite par une fièvre paludéenne. C'est un médicament polychreste par excellence."

The section "De la Medication Antimoniale" is much shorter than the preceding, but it is not inferior to it in any other respect; "Parallèle entre L'Arsenic et L'Antimoine." M. Papilaud is of opinion that antimony and arsenic possess an analogous action against those affections which are characterized by a functional perversion of innervation, as, for example, chorea, delirium tremens, hysteria, and others. Hence, he concludes, "cette action des ces medicaments est *régulatrice de l'innervation.*"

The next section treats "Des Maladies du Cœur;" and we pass on from it to the last section, which is headed "Application des Medications Arsenicale et Arsènio—Antimoniale aux Maladies du Cœur."

This part mainly consists of cases of various heart affections, some organic, some functional, but mostly treated on his plans of combining the two remedies, of supplementing one by the other, of alternating them, and of giving instead of one or both the salt called arseniate of antimony, which last he states in his "médicament usuel contre les maladies du cœur caracterises par les palpitations, la dilatation et l'hypertrophie." His ordinary dose is two milligrammes daily. The salt is prepared in granules, of which two may be taken in the morning, and two in the evening.

M. Papilaud is well read on his subject, and very enthusiastic as to his success, which appears to have been considerable. We should like to have the experience of other observers as to their success in cases of these kinds treated in like fashion; but meanwhile, we must heartily commend M. Papilaud's work, for it is a credit to our profession.

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1 *Essentials of the Principles and Practice of Medicine. A Handy Book for Students and Practitioners.* By HENRY HARTSHORNE, M.D., Professor of Hygiene in the University of Pennsylvania. Philadelphia: Henry C. Lea. 1867. Pp. 417.

2. *Pathological Sketches.* By J. H. JAMES, F.R.C.S., &c. London: John Churchill & Sons. 1867. Pp. 102.

Dr. HARTSHORNE'S work very much resembles, both in its design and its arrangement, the wonderfully comprehensive little volume published some years ago by Dr. Tanner, as a Manual of the Practice of Medicine, the last edition of which has expanded into a bulky octavo. Dr. Hartshorne has, however, devoted a larger portion of his book to general pathology, and to a general consideration of the symptoms and signs of disease, and the principles of treatment; these subjects occupy the first 120 pages, the remainder being devoted to special pathology and practice. He gives us a succinct statement of the most recent doctrines in pathology, and a

discriminating account of the various methods of treating disease which have been adopted; he shows a thorough acquaintance with the literature of medicine, and with the actual difficulties of practice. It may be questioned how far the science and practice of medicine can be usefully brought before the mind, brought in such a manner as to leave a true impression, except at considerable length; but as far as this is possible, Dr. Hartshorne has done it, and to those who want a handy-book, and can usefully assimilate such concentrated information, we recommend it without hesitation. As a fair specimen of the book, and deserving of attention on account of the opportunities which American physicians have had of observing the disease we subjoin his article on diphtheria:—

"DIPHTHERIA.

"SYNONYMS.—*Pseudo-membranous Angina; Putrid Sore Throat; Diphtheritis.*

"HISTORY.—Though the name diphtheria (from διφθερα, a skin or membrane) was only given to this disease by Bretonneau of Tours about forty years ago, it appears to have been described by Aretæus of Cappadocia as a disease of Egypt; and was mentioned also by Macrobius and Cœlius Aurelianus among early writers. Hecker gives an account of its prevalence in Holland in 1337; Carnevale, at Naples, 1620; Tamayo, at Madrid, called it *garotillo*, in 1622. Ghisi first clearly described the pseudo-membranous formation, at Cremona, 1740. In France, Chomel saw it in 1743-9; in England, Fothergill, in 1754; Douglas of Boston, in this country, in 1736, and Samuel Bard, of New York, in 1771. Huxham, Cheyne, Rosen, Albers, and Guersent also described it under different titles. Bretonneau most fully made it out as a distinctive disease, in 1826. Since that time it has been recognized and treated of by nearly all medical authorities.

"Late epidemics of it have been, principally, those of Paris and Boulogne of 1855-7, passing to England in the latter year; and of our own country, beginning in California in 1856, and in the Eastern States a little later, gradually increasing in prevalence until 1860. Since that time it has declined in frequency, although still existing, and sometimes attended by great local fatality. Bretonneau, not unreasonably, supposes Washington and the Empress Josephine to have died of diphtheria. Stephanie, the beautiful queen of Portugal, and Valleix, the eminent French physician, were victims of it.

"VARIETIES.—1. Simple; 2. Croupous; 3. Ulcerative; 4. Malignant diphtheria.

"SYMPTOMS.—Premonitory, but not distinctive, are general *malaise*, slight sore throat, and swelling of the lymphatic glands behind the jaw.

Then, in the simple form, fever occurs: with headache, furred tongue, constipation, and difficulty of swallowing. On examination, a swollen and very red or purple appearance of the fauces will be observed, as well as of the palate and tonsils. Over one or both of the latter, there may be seen, often as early as the second or third day, a whitish or yellowish white membranous deposit. All the symptoms continue, in this form, from five to nine days; when, in favourable cases, convalescence follows.

"The croupous form has caused the greatest number of deaths, especially in children. This seems especially prone to follow measles or scarlatina. In it, after the same early symptoms as those above described, but sometimes violent from the beginning, increase of discomfort in the throat is complained of. Then an abundant yellow or brownish leathery exudation is found to cover the tonsils and fauces; which, under the exudation, are much swollen. Often quite early in the attack, the pseudo-membranous inflammation extends to the larynx. This is shown by the usual symptoms of croup; the barking cough and voice, and difficult inspiration, becoming whistling or sibilant when the obstruction to breathing is the greatest. A fatal termination may occur, by asphyxia, in a very few days. This can only be averted by the detachment and expulsion of the membrane, without its re-formation.

"The ulcerative variety is not common. When destruction of the palate and tonsils has attended it, with copious dark-coloured and pulpy exudation, and some extravasation of blood, it has been mistaken for, and described as, gangrene; whence the old name, 'putrid sore throat.' The occasional existence of true gangrene cannot be altogether denied.

"MALIGNANT DIPTHERIA.—At the commencement of this, there is, with intense headache, not unfrequently *vomiting*, which is uncommon in the milder varieties, and hemorrhage from the nose, mouth, stomach, or rectum. Great dysphagia soon exists, and enormous engorgement of the submaxillary, parotid, and cervical glands. The tonsils, pharynx, and palate are covered thickly with a leathery deposit, at first yellowish, but soon becoming ash-coloured, brown, or almost black, and of an offensive odour. The tonsils may suppurate or even slough. The nostrils are also sometimes involved; being swollen, lined with false membrane, and emitting an acrid and fetid discharge. Extreme prostration comes on, at a more or less early period; it may be from the first day. The pulse becomes very rapid, the face lividly pale, morbid heat of the skin being followed by clammy coldness. Coma often precedes death. The latter may take place in three, four, or five, occasionally in one or two days; sometimes from the constitutional impression of the disorder, before the local affection has been fully developed.

"SPECIAL SYMPTOMS AND COMPLICATIONS—*Albuminuria* is present in most severe cases of diphtheria, from an early time in the attack. A diphtheritic affection of the *skin* has been now and then observed. A

blistered or otherwise abraded surface will usually, in the course of the disease, be covered by false membrane. *Pneumonia* is an occasional and dangerous complication.

"Sequelæ.—These are, especially, long-continued debility, paralysis of the soft palate, and general paralysis in various degrees. In the last of these, deglutition, articulation, vision, and locomotion may be involved. A fatal result may occur after a few weeks, or recovery after a longer period; sometimes from two to eight months.

"Morbid Anatomy.—The pellicle or deposit, formed upon the highly injected and tumefied mucous membrane of the fauces and throat, constitutes the anatomical peculiarity of the disease. Minutely examined, the false membrane is found to vary from $\frac{1}{20}$ to $\frac{1}{8}$ of an inch in thickness, and to be fibro-laminated, *i.e.*, of layers of fibrinous net-work, including epithelial cells, and having on its free surface exudation corpuscles or 'pyoid globules,' and granules; these forms appearing to be only stages of degeneration. No process of organization or development occurs in the mass; it is aplastic. In some cases only a granular superficial infiltration of the mucous membrane is observed, without even distinct fibrillation.

"The common deposit of diphtheria differs from the false membrane of simple inflammatory croup, and still more from the 'coagulable lymph' of inflamed serous membranes, in being thicker, more tough, yellower, and less capable of anything like organization.[a] (Dr. B. Sanderson asserts that he discovered evidence of development of the exudation in one or two specimens of the simple form of diphtheria.)

"Pathology.—Excluding from the title of diphtheria all instances of accidental or merely inflammatory 'diphtheritic' or pseudo-membranous formations, as they occur, for example, in croup and scarlet fever, we must admit that there is a special zymotic or 'enthetic' disease, for which that name is appropriate, and should be reserved. It is a toxæmic or 'dyscrasial' affection, in which the morbid change in the blood has its main and characteristic local manifestations in the throat.

"Causation.—Not doubting the existence of a special material cause yet unknown except by its effects, we can only say further that the disease is generally epidemic or endemic, with a special tendency to limited localization. It acts with intensity in confined centres; as, a small village, a crowded school, a numerous family; inflicting therein often a terrible loss in proportion to the members attacked; a sort of domestic pestilence.

[a] Bretonneau long since, and Dr. Sanderson a few years ago, imitated the diphtheritic exudation, by injecting oil of cantharides into the throats of animals. The principal difference was in the manifest tendency to organizatian in the cantharidal pseudo-membrane.

"Is diphtheria transmitted by contagion? I incline to believe that it sometimes is so, although clearly not dependent upon that mode of propagation in its epidemic migrations. The certain examples of its extending from one person to another are few; but I think I have known of one at least.

"Children are much more liable to diphtheria than adults. Climate and season do not seem to affect its prevalence. Nor does it show any very decided preference for otherwise unhealthy places, where filth or crowd-poison abounds. Even its promotive causation, then, seems to be at present less known than that of most other diseases.

"DIAGNOSIS.—From *scarlatina*, diphtheria is distinguished by the absence of the eruption, and of the peculiar punctated or brick-dust like flush of the throat, and 'strawberry' tongue. That scarlet fever *predisposes* to diphtheria, as a subsequent attack, is a well established and not unimportant fact.

"With *membranous croup*, it is contrasted in the following manner:—That disease is a sporadic and sthenic local phlegmasia, whose general symptoms are, as much as in any inflammation, dependent upon the local affection; while diphtheria is a constitutional disorder, usually epidemic, in which the local symptoms are secondary. More directly, in practice, we may mark the commencement of the pseudo-membranous deposit, in diphtheria, about the tonsils and pharynx; in croup, in the trachea or larynx. That of diphtheria rarely extends, in any case, below the larynx; that of croup, not unfrequently even into the bronchial tubes. After the laryngeal complication or extension has occurred in diphtheria, the croupal symptoms are really the same as those of any other laryngeal obstruction, and thus are not different from those of croup.

"From *thrush* and *aphthæ*, diphtheria is known by the deposit being much larger and thicker, never vesicular, and mostly duller in colour; and attended generally by more severe constitutional symptoms. Thrush begins in the mouth; it is, moreover, much more uncommon in adults than diphtheria; and is never epidemic.

"PROGNOSIS.—*Simple* diphtheria is not very dangerous to life. The croupal form is decidedly so; and the malignant is fatal in a large majority of cases. *Insidiousness* is a trait often belonging to the disease in children; a name which has been applied by some, for that reason, is 'creeping croup.'

"TREATMENT.—No specific remedy having been discovered for this disease, we must be governed in our tentative treatment of it by our idea of its nature; while concluding upon its therapeutics, finally, through experience. Nothing, it may be confessed, is very satisfactory, as yet, in the management of bad cases of it. All agree that it is not a mere local inflammation, but a systemic affection primarily; and that its type is most generally asthenic. Much depletion is therefore not to be thought of. I

would never bleed from the arm in diphtheria. In simple, open cases, I have used leeches to the throat, with seeming decided advantage within the first three days; even their use, however, must be exceptional. Moderate purgation, as with citrate of magnesia, or Rochelle salt, at the very beginning, is well in the simple and croupal, though not in the malignant form.

"Chlorate of potassa is a favourite medicine with many in this disease. My best results in bad cases have attended its early and free use. An adult may take twenty grains in solution every three hours; I have given five grains every two hours to a child five or six years old.

"Tincture of chloride of iron is relied upon by some; from ten to twenty drops every three hours for an adult, with or without the chlorate of potassa. Sulphate of quinine is also given, alone, or at the same time with the above remedies, by a number of practitioners; say of quinine, for an adult, a grain every two or three hours.

"Besides these, or instead of them, for internal use, permanganate of potassa has, after some trial, the recommendation of one or two observers. A drachm of it may be dissolved in a pint and a half of water, a fluid-drachm of this being taken every hour. Sulphite of soda, ten grains every two or three hours, is worthy of trial in this, as in other zymotic diseases.

"Concentrated liquid food must, as a rule, be given throughout an attack of diphtheria; milk, beef-tea, and very often wine whey or brandy or whiskey punch; in small quantities at short intervals, according to the degree of prostration present.

"Local treatment is, by most physicians, regarded as very important. Experience has shown, I think, that it ought not to be violent. Ice in small pieces melted in the mouth slowly, is probably as useful as any application. Muriatic acid and honey, equal parts, applied freely with a large camel's hair pencil; or diluted with water and used as a gargle, I believe to be serviceable. Creasote dissolved in glycerin,[a] lime-water; chlorinated soda dissolved in twenty parts of water; and permanganate of potassa, a drachm in a pint, make also appropriate gargles. In a young child ice is often the only local application possible without a struggle so disturbing as to make the benefit of it doubtful. Cold water compresses may be applied outside of the throat in the early stage, while there is excess of heat. Later, flannel wrung out of hot water to which an equal amount of spirits or vinegar has been added, will give more comfort.

"Inhalation of the steam of lime-water is worthy of trial in diphtheria, especially in the croupous variety; or, the *atomization* of lime water by the *nephogene* or some other apparatus constructed for the purpose.

"But, I believe the local treatment to be, after all, secondary. And

[a] Creasote, 4 to 8 drops; glycerine and water, of each 2 fluid ounces.

especially is the effort (which I have seen practised) to remove the patches of exudation by force, as by excision or actual cauterization, to be deprecated, as likely to do harm rather than good."

At page 75 we notice a slight inaccuracy—"the heart's apex strikes during the impulse at a point just below, and outside of the left nipple." Now, as observation of the exact position of the apex often gives us valuable aid in the diagnosis of thoracic disease, our topography should be as correct as possible, and the true position of the apex-beat is in the fifth interspace, and somewhat against the sixth rib, midway between the left edge of the sternum and the nipple line.

Mr. James, the author of the second book on our list, has been for some forty years in practice in Exeter; his work consists of two parts; the first is devoted to the subject of inflammation, on which he wrote in 1821. He complains that pathologists have of late years been too much in the habit of concluding that the phenomena observed under the microscope, when transparent tissues were irritated for the purposes of experiment, are the phenomena of all inflammations. These he divides into the limited, the diffuse, inflammations with tendency to ulceration or mortification, and resolving inflammations. Speaking of sloughing sores, he recommends the application of equal parts of strong decoction of bark, and good port wine. Many, he says, seem to think that simple alcohol possesses all the virtues of good old port—not only does the stomach of an invalid, but the surface of an ulcer, know the difference. The second part of the volume treats of bleeding, and change of type; and on these points it is interesting to have the opinion of one actively engaged in professional duty for so long a period; the type of disease has changed, he says. On referring to his case books, he finds that soon after 1837 he began to bleed much less frequently, and he did so, not because of anything which had been written against bleeding, but because he ceased to meet with cases demanding it. Bleeding from the arm, he thinks, may still be useful in the diseases of a population who have not been reduced by the great town system; to be useful, however, he reminds us, bleeding must be practised at the beginning of the malady, and in such a manner, that the blood flows in a full stream. Discussing the change in the constitution of the people, he suggests that in addition to the bad air and bad habits which injure the population,

something is to be attributed to the present plan of feeding the animals designed for human food, in a manner which, though it produces bulk, does not produce healthy structure.

Handbook of the Sphygmograph: Being a Guide to its use in Clinical Research. To which is appended a Lecture, delivered at the Royal College of Physicians, on the 29th of March, 1867, on the Mode and Duration of the Contraction of the Heart in Health and Disease. By Burdon Sanderson, M.D., F.R.S., Physician to the Hospital for Consumption; Assistant Physician to Middlesex Hospital; and Joint Lecturer on Physiology in the Middlesex Hospital Medical College. London. 1867. Pp. 83.

In this country, at least, the sphygmograph has been introduced to the profession under very favourable auspices. Several able observers have devoted themselves to testing the utility of the instrument, and their reports have, from time to time, been published. The sphygmograph has, indeed, been exceptionally lucky in the treatment it has received, and if its use as an instrument of clinical research fails to become general, it will certainly be from no want of careful investigation. Our readers have already been presented with an analysis of the first English work on the subject, and have also had the opportunity of perusing Dr. Grimshaw's valuable researches on the peculiarities of the pulse trace in fevers. The appearance of Dr. Sanderson's little book, we accept as another token of the recognition of the great importance of applying to the solution of the complex problems of our science, every instrument which can render our knowledge more exact. In our observation of the phenomena of the circulation, the sphygmograph promises us a precision which the *tactus eruditus* of a great physician occasionally approached, but to which many veterans in our armies of the past never attained.

The handbook of the sphygmograph consists of three chapters, and the lecture referred to in the title. In his preface, the author says—"Those who require only a grammar of the sphygmograph will, I trust, find what they want in the first two chapters." In the third chapter we have "Notes on certain causes and complications of disorders of the circulation;" and "the more general questions

relating to the physiological pathology of the circulation which are connected with the movements of the heart and arteries, are discussed" in the lecture which is placed at the end of the volume.

In the first chapter, after the usual description of the instrument, there are sections treating of its defects, modifications, and the mode of application. We find that Dr. Sanderson, like other observers, has discarded the metallic wings, and recommends, for fixing the sphygmograph on the arm, that to the carpal end of the brass framework there should be adjusted "a rectangular block of brass, by the under surface of which it rests on the tendon of the *flexor longus pollicis*, and on the space between that tendon and the spine of the radius; the block being kept closely applied to the surface by means of a strong elastic band, which encircles the wrist. By this method the framework of the sphygmograph is made to rest firmly on a surface of bone, so that the axis of the lever is maintained at a constant distance from the artery." The movements of the forearm are controlled by the use of Mr. Berkley Hill's pad. Further on we have a description of the author's mode of regulating the pressure, but it is not so clear as it might be, and a diagram would be a great assistance in enabling the reader to follow the author's meaning. The uncertainty which attends the registration of the movements of the lever on paper, has led to the use of smoked glass. This substitution, which has been long used by many observers, renders the registration more certain, and yields more accurate results, for the point of the pen never fails to leave its trace; and, what is more important, the friction between the pen and the glass plate is reduced to a minimum. Wolff, in his numerous experiments, used smoked paper, on which the trace can be easily fixed by a little alcohol, or oil of turpentine. This plan is a very useful one, and we are surprised to find no mention made of it by one who has been so careful a student of Wolff's able treatise.[a] The trace can be easily fixed on glass by means of photographers' varnish; but, still the ink and paper, as used by many, is so much more convenient, and, with care, admits of being rendered so accurate, that we believe it will continue to be used by many.

The second chapter contains some useful remarks on the "terms usually employed in describing the character of the pulse, as felt at the wrist." These will well repay perusal, and show, in a striking manner, how useful the sphygmograph can be made, in imparting a

[a] Characteristik des Arterienpulses. Leipzig. 1865.

knowledge of the varieties of the pulse. The description of the "Arterial movements which constitute the pulse," comes next, and it is in this part of the chapter, which it is most essential the student should thoroughly understand, that many difficulties will be found, arising from the author's defective arrangement of his materials. We would advise any who read this chapter, to do so only after a careful perusal of the lecture, wherein will be found some definition of the terms employed in describing the parts of each pulsation.

The obscurity which pervades the second chapter, is also due, in part, to the fact that Dr. Sanderson endeavours to introduce a new nomenclature of the parts of the pulse-trace, differing from both that of Marey and that of Wolff. It is much to be regretted that there should be a want of uniformity in the terms employed to designate the various curves of each pulsation, as the student of sphygmography must now learn each method before he can fully avail himself of the literature of the subject. To the reader of Marey, the technicalities employed in this little book are at first very perplexing, and although we must admit that this is, in part, due to the more correct views we now have of the successive phenomena which form each pulsation, yet it chiefly arises from the author's method of referring to the movements of the artery, instead of speaking of curves of the trace. We must deprecate this introduction of new technicalities, especially as the inventor of the sphygmograph has given us a much more simple nomenclature. As revolutions are most destructive to the prosperity of newly constituted states, so are unnecessary alterations in its language most fatal to a new method of inquiry.

The third chapter contains much useful information on the pulse-form in various disorders of the circulation—*Mitral valvular disease*, *Aortic valvular disease*, *Rheumatic carditis*, *Increased arterial resistance; Respiratory and pulsatile movements of the veins of the neck*, and the *Influence of consciousness on the circulation*, are each, in turn, briefly treated of. In speaking of mitral regurgitation, it is stated, "the *irregularity* met with in mitral disease, possesses a character not before noticed by writers, which affords a key both to its cause, and the only mode of remedying it. In the irregular pulse of mitral regurgitation, the contractions of the heart may be divided into two classes—those which are frequent, short, and ineffectual, and those which are long and forcible. And it is found by observation, that the former occur during inspiration, the latter during the respiratory pause." This is an important observation, but by no means so

original as the first part of the quotation would lead us to suppose; for on turning to Marey's great work we find, in a note to his observations on this subject, the following passage:—

"Il nous a semblé que, dans certains cas le rhythme de la respiration influe sur le retour des irrégularités du pouls dans l'insuffisance mitrale." [a]

We regret that this passage escaped Dr. Sanderson, to whom we must allow the credit of giving an explanation of the phenomena which the acute intellect of Marey anticipated. The following is our author's explanation of the rhythmical character of the irregularity of the pulse in mitral regurgitation:—

"The mechanical effect of inspiration, as I have elsewhere shown, is to augment the quantity of blood contained in the pulmonary circulation, and hence to increase the frequency of the contractions of the heart. This increased frequency depends on the distended state of the auricles, in consequence of which the ventricles fill more rapidly during their period of relaxation. In this way the length of the diastolic pause is diminished, and the hurried action of the heart satisfactorily accounted for; but the question still arises, why are the rapid beats which occur in inspiration, also ineffectual? Very probably because the mitral valve does not close; the heart being distended with blood, its walls are kept apart to such an extent, that the curtains do not meet. The ventricle contracts, but much of its blood is discharged into the auricle, to be returned to the ventricle as soon as its contraction is over. It is not until the effect of inspiration, in keeping the auricles full, ceases, that the curtains get near enough to allow the heart to make an effort sufficiently effectual to send a full tide of blood into the aorta, and thus relieve the distended pulmonary circulation." [b]

Of the lecture, which forms the concluding portion of this small book, we need say little, as it has already appeared at length in the pages of a weekly contemporary. It is, in our opinion, by far the best part of the *brochure* before us, and if the matter it contains had been embodied in the second chapter, our author would have produced a most useful "handbook." As it is, those who are already well acquainted with the use of the sphygmograph will find in this volume much to interest them.

[a] Physiologie Médicale de la circulation du Sang. Paris, 1863. Note on page 524.
[b] Croonian Lecture, 1867. Proceedings of Royal Society and Phil. Trans.

Intestinal Obstruction. By William Brinton, M.D., F.R.S. Edited by Thomas Buzzard, M.D., London.

This valuable little volume is virtually an extension of the Croonian Lectures delivered by Dr. Brinton before the College of Physicians, London, in the year 1859.

It claims attention, not only on account of the deserved celebrity of its lamented author, but also because of the very meagre description of this class of disease found in most of the works of medicine now extant.

It possesses also, in many points, the charm of originality. The views herein propounded as to the cause of the fecal vomiting observable in cases of intus-susception, and other obstructive diseases of the intestines, are wholly the conception of the author, and directly contravene those held from the day of Galen to the present; and it could not be thought superfluous to republish these lectures in a connected form, when we find that most of the text-books on practical medicine published during the twenty years which have elapsed since the ideas held by Dr. Brinton were first propounded, omit all notice of the doctrines which are here reiterated.

It should be borne in mind also that these are no mere deductions from, or compilation of, the work of others; but the result of the labour and untiring energy of an observer of disease, whose one aim was to search after truth.

The work consists of three chapters; the first, in the form of an introduction, contains a discussion as to the kinds of obstruction, after which a typical case is presented for our study, in which the symptom of fecal vomiting is selected for priority of consideration. The anti-persistaltic view is stated and refuted. Dr. Brinton's own explanation is put forward, and, to our satisfaction, established. The stages, and symptoms of each stage occupy the remainder of this chapter. The chief varieties of intestinal obstruction, their absolute and relative frequency, and their causes, form the subject of the second chapter; while the third is taken up by a minute consideration of the treatment to be adopted in these obscure and often dangerous cases.

Having thus given a short outline of the work, we shall proceed to consider it more in detail.

Obstruction to the transit of the contents of the intestinal tube, "may depend on two different, and, in some sense, contrasted

causes;" it may be due "to a failure of propulsive energy on the one hand, or to an excess of resistance to such energy on the other." It is evident that all those cases which arise from paralysis of the nerves which supply the intestinal muscle, as well as those depending on weakening, paralysis, or destruction of the muscles themselves may be included in the first category; while those cases producible by occlusion of the calibre of the tube from without, by impaction of the contents, by constriction or intussusception, are to be referred to the second class.

Dr. Brinton illustrates these several cases by reference to experimental physiology. If in one rabbit the œsophagus be tied; in another, it be completely cut across; while in a third, division of the pneumo-gastric nerves be effected; in all, the effect, differing somewhat in degree, will be the same—the accumulation of the food, when administered, immediately above the seat of the injury.

He objects to distinguish these two forms, by applying to the one, and withholding from the other, the epithet "mechanical," for the impediment due to a mere failure of contractile power, is quite as mechanical as any physical obstacle impacted in the bowel."

Having thus disposed of the preliminary discussion as to the applicability of the term to all forms of the disease, our author proceeds to the discussion of—(1.) The pathology of intestinal obstruction generally. (2.) Its chief varieties. And (3.) The principles of its treatment.

In dealing of the first portion of the subject, a typical case is sketched as follows:—

"A person, perhaps hitherto healthy, experiences a sudden constipation, attended with disproportionate uneasiness or flatulence, soon merging into pain and distension of the belly, with violent rolling movement of the intestines. The distension increasing, nausea and vomiting supervene; and, gradually becoming more frequent, end by rejecting, not merely any casual alimentary contents of the stomach, or the greenish, bilious, alkaline fluid commonly thrown up when this organ is unoccupied by food, but a fluid of greater opacity, colour, and consistence, with a distinctly fæcal odour. A further aggravation of these symptoms now conducts the malady to its termination. This, if fatal, is usually preceded, locally, by signs of paralysis, inflammation, or even rupture of the distended bowel, and, constitutionally, by exhaustion or collapse replacing a febrile reaction. In other cases, the obstacle being removed by Nature or Art (if by the former, rarely before life is in extreme danger), the symptoms subside with comparative celerity. The pain, distension, and vomiting cease;

the bowels are relieved by copious stools; and the patient (if not placed in further peril by any of those conditions incidental or consecutive to obstruction just hinted at) is rapidly restored to comparative health."

Of the symptoms here delineated, that of stercoraceous vomiting is selected for priority of consideration, on account of its pathognomonic character, and because its occurrence had been previously explained, even from the earliest periods in the history of medicine, by the doctrine of anti-peristalsis.

In fact most of the illustrious fathers of the profession stated this doctrine in the most dogmatic manner, as if its truth did not admit of doubt. Sydenham, for example, when treating of Iliac Passion, thus expresses himself:—"*The peristaltic action of the bowels is reversed*, so that purges and clysters are thrown up like emetics, and the feces of the bowel are passed through the mouth and throat." In treating of the bilious colic which prevailed from 1670–1672, he thus writes:—"This symptom (constipation) proceeded until its violence determined *a total inversion of the peristaltic motion and ileus;* then are cathartics transformed into emetics, and even enemata are carried upwards through the whole tract of the intestines, and expelled by the mouth, feces and all."

Even at the present day, the same view is taught in some text books; thus, in the second edition of Dr. Aitken's *Practice of Medicine*, published in 1863, and at page 924 of the second volume, "Ileus is a severe variety of colic, accompanied by vomiting (with or without constipation) which is often so obstinate that *the action of the bowels is inverted*, and fecal matter is thrown up by the mouth."

We shall offer no apology for laying before our readers a full analysis of our author's views on this question, and, in the first place, in refutation of the old theory, the following arguments are advanced:—

"1st. Although an anti-peristalsis is often affirmed in the writings of numerous authors, in not one instance has its occurrence been substantiated by observation.

"2nd. The results of vivisection in animals in whom artificial obstruction has been produced, are directly opposed to such a theory, inasmuch as the ordinary movements, far from being reversed, are much more violent than usual; the same may be verified by inspection and palpitation in the obstructed intestine in the human subject.

"3rd. Over-irritation at the seat of obstruction is said to be the cause

of the supposed anti-peristalsis, but this state exists in numerous cases of intestinal disease, where no such effect as fæcal vomiting is the result.

"4th. The necropsy of cases of obstruction positively refutes the notion of anti-peristalsis. The portion of the tube most distended is that at the seat of obstruction, and the intestine tapers from this towards the duodenum; the reverse ought to be the case if the supposed theory were true.

" 5th. While the supposed anti-peristalsis might fairly be expected to extend, like the irritation causing it, beyond the occluded point, both observation and experiment show that, long after the occurrence of obstruction, the bowel at and below the occluded point often empties itself by propelling its contents in the normal direction. So that, on the theory of an anti-peristalsis, a single irritation applied to a given part of the bowel renders it the starting-point of two precisely opposite movements—one upwards towards the stomach, one downward towards the rectum. And while, as above noticed, the first of these supposed movements not only fails to empty the segment it starts from, but always permits its extreme distention; the last, on the contrary, often empties and contracts its corresponding segment of the intestine to a tube with a thick wall and a narrow calibre, like the stem of a tobacco pipe.

" 6th. A comparison of the symptoms and appearances in some cases of obstruction affords a specific disproof of all anti-peristalsis above the occluded point. In spite of the persistance of fæcal vomiting, castor oil, crude mercury, and other substances easily identified, which have been taken into the stomach shortly before death, are shown by the necropsy to have traversed the whole intervening length of intestine, to be stopped only by their reaching the strangulation itself."

Having thus most thoroughly, we think, refuted the old doctrine, he proceeds to state the theory which he was led to form from observation and experiment, and here we shall give his own words:—

" The movement proper to the healthy intestine is a circular constriction or peristalsis, which, travelling slowly and intermittently down its muscular wall, propels its contents in a direction from the stomach towards the anus. And when any part of the intestine has its cavity obliterated by an immovable mechanical obstacle, its contents, propelled by such a peristalsis, are stopped at the obstructed point. Here they gradually accumulate, so as first to fill, and then to distend, a variable length of the canal, with a more or less liquid mass. But a peristalsis, engaging the wall of a closed tube filled with liquid, and falling short of obliterating its calibre, sets up two currents in that liquid: one at the surface or periphery of the tube, having the direction of the peristalsis itself, and one in

its centre or axis, having precisely the reverse course. Those particles of the liquid which are in contact with the inner surface of the tube are propelled onwards by the muscular contraction of its wall. And this propulsion is necessarily accompanied by a backward current in those particles which occupy the axis or centre of the canal."

Two diagrams are given pointing out the truth of these ideas by exhibiting the nature and direction of currents produced in an inflexible closed tube, filled with liquid, and fitted with a perforated septum, capable of moving freely along its interior. When the septum is moved towards the closed end of the tube two continuous currents are established—a peripheral of advance and a central of return—to and from its closed end respectively.

Circumstances, such as the flexible and elastic nature of the intestinal walls, the variable intensity, rapidity, and frequency of their muscular contractions, &c., may concur to interfere with the exactness of these two currents; which would also be modified by the degree of liquidity of the intestinal contents, and the distance of the seat of obstruction from the pylorus; by the dilatability of the obstructed bowel; by the extent of paralysis which, sooner or later, is the result of distention; by the nature of the ingesta, and by the kind of treatment pursued. The fecal vomiting may then be explained as follows:—

"The appearance of these matters, and the subsequent necropsy, often conclusively show that they have traversed a certain length of intestine in a direction towards the stomach; that they have returned, for example, to this organ from an obstruction seated in the lowest part of the ileum, or even in the colon. This reflux is the result of the intestinal peristalsis; which, acting on an obstructed and distended bowel, not only effects the ordinary propulsion of its contents towards the obstacle, but also gives rise to what is, theoretically, a backward current in the liquids occupying the centre of the tube; practically, a *tendency* to such a current. However interfered with by other movements, abdominal or intestinal, this tendency has sufficient energy to effect a more or less intimate mixture of the intestinal contents; and to return some of them from the obstructed part, to a higher segment of the canal: generally, indeed, to the duodenum or stomach, whence they can be expelled by vomiting."

In addition to those circumstances mentioned above as capable of altering the date at which fecal vomiting follows obstruction, it will be found that where the lesion is situated beyond the ileo-cecal valve, it will be much more delayed, in proportion, than where the

obstruction is in the small intestines. This, Dr. Brinton proves to be due to the length of time necessary to distend the ileum and cecum so as to allow of the backward passage of feces by inducing that patulous state of the ileo-cecal aperture, which is its necessary preliminary condition.

Where the obstruction is far below the valve, the stercoraceous vomiting may even be delayed so long that death ensues before it can occur.

It may well be asked how did such a theory as anti-peristalsis hold its ground for so long? Why did it satisfy the minds of men like Good, Cullen, and Sydenham? We imagine it has arisen partly from the paucity of cases of fecal vomiting and partly from the plausibility of the theory. Its truth was never questioned, but was always assumed; and, supposing it to be true, it satisfactorily accounted for the symptom. Still we cannot but wonder that some deep thinkers were found to give credence to the astounding proposition, that in some cases of ileus, the enemata introduced into the rectum, were drawn up through the entire length of the intestinal canal and rejected by the mouth. We are compelled to attribute such mental obliquity to excessive credulity in relying on the statements of ignorant patients, who will often make the most absurd misrepresentation of facts in detailing their maladies.

The next point considered is the rapidity of death, and this is said to depend on the degree and rapidity with which the segments of the intestine, above the point of obstruction, become filled and distended, whether by inflammation, secretion, or ingestion.

Dr. Brinton makes a division of the ordinary course of intestinal obstruction into two stages. In the first stage, the disease continues in a modified state the normal action of the bowel; it is capable of cure, and is characterized by pain, distension, vomiting, and writhing peristalsis; in the second stage, the action of the bowel is arrested, and, most generally, annihilated, and this state of paralysis leads, in the majority of instances, to fatal enteritis or peritonitis. Among the phenomena of the first stage, the *distension* of the intestine is looked upon by Dr. Brinton in the light of a *physical sign*, "far more conclusive than any mere symptom, and which may be verified before they become prominent."

"Experience entitles me to assert, that the accumulation of intestinal contents immediately above the obstructed point may sometimes be detected, as a slight fulness to palpation, and a much more definite dulness

to percussion, where many of the other indications of obstruction are scarcely perceptible or even absent."

Now, while we are unwilling to detract from the importance of this sign, where it can be ascertained to exist, we are led to believe that too much reliance on its presence is an error in the opposite direction. We cannot agree with Dr. Brinton's views as to the almost equal value of physical diagnosis in the investigation of thoracic and abdominal diseases. There are too many modifying circumstances in the latter case which lessen the certainty and amount of the information obtainable. We lack the facilities for comparison, and we know that insuperable difficulties frequently arise from excessive thickness or irritability of the abdominal wall, or from tension, due to flatulent distension or over sensitiveness, which interfere seriously with palpation or percussion in this region. On the other hand, we do not wish for a moment to detract from the value of a careful manual examination of the abdomen, in many cases. When conducted with care it will frequently convey most valuable information, especially where by long experience considerable *tactus eruditus* has been acquired.

The other symptoms of the first stage are next considered. *Fluctuation* is regarded as being by no means an early symptom, nor one easily verified; the movements of the intestines (whence is derived the name volvulus often applied to these cases), are a valuable aid to diagnosis at an early period, and, if present, they can rarely be mistaken. Dr. Brinton states that even after the movements have ceased, the contour of the intestine may be marked out on the abdominal wall; but we believe that this may often be concealed by general distension. Of *pain* two kinds are distinguished; the one is shared by enteritis in common with obstruction; it is often the earliest symptom and is either sudden and violent or attains its maximum within a minute or two after the occurrence of obstruction.

"It is usually intense in intus-susception, and in the impaction of gall-stones; somewhat less marked in the obstruction produced by twisting of the bowel, or by bands and adhesions; scanty in the obstruction of stricture; and almost absent in the obstruction caused by the impaction of fæces in the large intestine."

A curious explanation is offered of this kind of pain. It is said to be due to the distension of the minute vessels of the part, produced by the obstructing cause, through the medium of the

sympathetic nerve. We imagine that this might also be offered as a very rational explanation of the pain, often so intense, observed in many of those cases of cerebro-spinal blood poisoning which have occurred during the last two years. The other species of pain is more frequent and characteristic; it is the pain of intestinal distention which it immediately follows, and to which it is directly proportionate in degree; it is paroxysmal and has been compared, as to its mode of genesis, to cramp in the leg, but this is shown to be improbable, as the pain is often most intense when the distension is so great as to produce paralysis, and, on the whole, it is likely that this kind of pain owes its origin to a nervous, rather than a vascular source. The symptoms of the second stage—paralysis, inflammation, and collapse, are then passed under review; but as our author's views are those commonly held, we shall pass on to a brief consideration of the varieties of obstruction which is the subject of the second chapter.

From the analysis of 12,000 necropsies it is concluded that obstruction (exclusive of hernia) is the cause of death in 1 in every 280 cases of death from all diseases.

The relative frequency of the different varieties of obstruction is determined from an examination of about 600 deaths from causes verified by autopsies, viz.:—

"Intus-susceptions or invaginations, 43 per cent.; obstructions by bands, adhesions, diverticula, or peritoneum, external to the bowel, 31½ per cent.; strictures (including a few tumours) involving the intestinal wall, 17½ per cent.; torsion of the bowel on its axis, 8 per cent."

Of these, intus-susception, being the most frequent, is first discussed; they are believed to be, in the vast majority of cases, protrusions of an upper into a lower segment of bowel, and also single; "both in the sense that only one is present in the whole canal, and that, in this one, the inner and outer segments are continuous by a single intervening portion.

Dr. Brinton expresses his scepticism as to the actual occurrence of backward intus-susception as follows:—

"Without at all venturing to assert that a backward intus-susception is impossible, I feel bound to record the general impressions derived from unusually large experience as a teacher of anatomy, on the one hand, and special research into this very point on the other, by saying that any one will supply an important pathological detail who will dissect out *in situ*, and adduce, with proper confirmative evidence from a professed anatomist,

a single case of backward intus-susception, as usually understood. At present, the occurrence of any such lesion is certainly not affirmed by evidences more numerous or more trustworthy than those which, to the complete satisfaction of many highly educated and intelligent witnesses, affirm the existence of sea-serpents, or assert the perpetual subjection of the disembodied spirits of saints and sages, to the curiosity and greed respectively of some of the most foolish and unscrupulous of mankind."

Can it be possible that our author was not familiar with the case brought forward by the late Professor Harrison, and published in the *Dublin Quarterly Journal* for August, 1848. In this case a portion of the descending colon protruded upwards into the transverse portion to the extent of about an inch and a half. The preparation may be seen in the Anatomical Museum of Trinity College, and an inspection of it will speedily convince the sceptic of the possibility of backward intus-susception.

In the great majority of cases (56 per cent.) the ileum and cecum are protruded into the succeeding large intestine; in 32 per cent. the small intestine is affected, and of these, in 25 per cent., the lesion is located in the ileum; in about 12 per cent. the colon is exclusively affected, while the rectum merely forms the outer layer of protrusions from above.

For an account of the varieties of intus-susceptions which arise from differences in the exact seat of the lesion, and for the circumstances which combine to render the obstruction in most cases complete, we must refer our readers to the original; suffice it to say, that it is, in a great degree, owing to the vascular congestion, swelling, and exudation which follow the primary lesion.

In those cases which proceed to a favourable termination, the result is brought about somewhat as follows:—In the ring of adhesive lymph which forms around the upper boundary of the inverted portion of the bowel, two circular lines of demarcation are formed, and by ulceration, or sloughing, or both, the invaginated portion of bowel is cast loose, and is finally expelled per anum. The ring of lymph completes the channel and prevents effusion of the contents of the bowel. This process is, on an average, complete in about eight days; it occurs $2\frac{1}{3}$ as often in the invagination of the small intestine, as in the ileo-cecal variety; and, while in the former instance, recovery is the rule, in the latter more than one-third of the cases are rapidly fatal; its attendant risks are thus enumerated:—

"Inflammation, and all its modifications, take even more than their wonted share in this variety of obstruction. Ulceration, gangrene, and rupture of the distended segment immediately above the invagination are thus brought about. Peritonitis runs rapidly to diffuse suppuration; or, more slowly, forms an abscess (as on the psoas or iliacus muscle), ultimately fatal. Adhesion of the external lymph conditionates some future strangulation; contraction of the cicatrix ends in stricture and obstruction; accumulation of hard indigestible food, or violent exertion, bursts the soft adventitious tube, replacing the expelled bowel, and lights up fatal general peritonitis. Or lastly, the patient sinks gradually into a state of exhaustion, too complex to analyse, but probably sometimes referable in part to the loss of secreting and absorbing intestinal surface."

The mechanism of intus-susception is thus stated:—

"The immediate cause of the displacement must be sought, not only in an abnormal violence, extent, and abruptness of that contraction of the transverse muscular layer, which is one element of peristaltic movement, but in a deficiency of that co-ordinate contraction of the longitudinal fibres which is an equally important (though less energetic) constituent of healthy peristalsis. The sudden and forcible constriction of a considerable length of bowel—fixed, as it were, behind, by the hitherto unrelaxed contraction from which it has extended, and bounded in front by a relaxed flaccid bowel (unstretched by the due contraction of its longitudinal fibres) must, as it suddenly diminishes the width of the segment it involves, simultaneously increase its length, and thus push a variable portion of it into the succeeding dilated segment. This explanation, which seems to be a simple and incontrovertible application of the laws of intestinal movement to the facts of intus-susception, refers the lesion to two causes—an active and a passive; one which thrusts the bowel forwards into the next segment, and one which permits this segment to receive it."

The presence of the tumour is, of course, a most valuable sign in the diagnosis of this variety of obstruction, and should always be most carefully sought for in any case where constipation is unusually obstinate.

Intus-susceptions involving the ileo-cecal segment, or the colon may be distinguished from those affecting the ileum only.

1. By the prominence of tenesmus in the first class.
2. By the greater size and fixation of the tumour.
3. The scantiness of hemorrhage.
4. The subordinate share taken by obstruction.
5. The frequent presence of the end of the invagination in the rectum.

The suddenness of its invasion, the acuteness of its pain, the movements of the intestine, the presence of the peculiar tumour, and the rapidity of its prostrating effect, are so characteristic that there will be little difficulty in the diagnosis from enteritis, on the one hand, or from the remaining forms of obstruction on the other.

We quite concur in thinking that the quantity of urine voided does not indicate the proximity of the obstruction to the stomach, and we believe that it is no criterion of the exact seat of the lesion; on the contrary, we know that in many cases, not a drop of urine has been secreted, where *post mortem* examination demonstrated that the obstruction was situate in different segments of the upper bowel and even more than once in the colon. It was a marked symptom in Professor Harrison's case, to which we have drawn attention above, where the intus-susception was in the transverse and descending colon.

Among the other interesting matters which occupy the remainder of this chapter, we may mention that of obstruction due to large gall-stones ulcerating a passage into the duodenum, and becoming impacted in the small intestine, the diagnosis of which is fully detailed; also the importance of digital examination in the diagnosis of strictures, and the value of the precise amount of the injections of warm bland liquid, in coming to a conclusion as to the site of strictures or impacted masses in the large intestine. "A pint is said, in an ordinary case in an adult male, to indicate a point not lower than the upper end of the rectum; a pint and a half, two pints, three pints belong to corresponding segments of the sigmoid flexure. The descending and transverse colon, accept a larger but more irregular quantity of fluid."

This portion of the subject is concluded by a very valuable table of the statistics alluded to in this chapter.

The question of treatment opens up an important field for discussion, and in this part of the subject the author has done good service to the much neglected, but, as it seems to us, vitally important science of therapeutics. He rightly maintains that any treatment to be successful must be founded on correct pathology and accurate diagnosis. We shall quote his words:—

"Not only would it be difficult to mention any group of maladies which better illustrates the unity of the Science and the Art of Medicine; but I would add, as the key to the following remarks, that scientific insight, and accurate and early diagnosis, have a specific if not unusual

value in respect to both branches of treatment. If the process of mechanical obstruction cannot be distinguished from that of enteritis, the Physician may search in vain for principles to guide his administration of food or remedies. And if one form of obstruction cannot be distinguished from another, the Surgeon can scarcely venture to operate with any reasonable chance of success."

Is gastrotomy justifiable in intestinal obstruction? This question is answered with a decided negative, when considered as a general question, upon grounds which must commend themselves to every unprejudiced mind; for its necessity must be demonstrated by a comparison of its results with those of the disease when not so treated; but this is impossible, as we have no reliable statistics as to the mortality, either in obstruction or in gastrotomy.

But if it be asked should not the operation be performed in a particular case, an affirmative may be given, for there are cases so desperate that this will afford the only chance of life, consequently the operation may be undertaken at a period of the malady when it will prove less dangerous. An example of the truth of this proposition is quoted in the case of Amussat's operation for artificial anus, and its great degree of success is attributed partly to the fact that the peritoneum is not wounded, but chiefly to the greater certainty of diagnosis in those cases to which this treatment is applicable. We attach even less importance to the first of these causes, as we think that there is abundant evidence that with proper precautions there is little, if any danger, in a clean incision through the peritoneum.

It being granted that there are suitable cases for the operation, it remains for discussion what are those cases, and what are the indications for its use. That it is not adapted to *intus-susception* is proved by reference to the facts recorded in the second chapter as to the frequency of its favourable termination, and by the consideration that the origin of a fatal issue in these cases is often the result of inflammation and exudation, which the operation could never remove, even if performed at an early period. Those cases arising from *strictures* and *tumours* are proved to be more adapted to Amussat's operation, which is palliative, yet safe and easy. In obstruction due to constipation or impaction of gall-stones, the patient's chance of recovery can never be increased, and, it may be, diminished by an operation.

Where, however, the impediment to the transit of the intestinal contents is due to "a band of organized lymph, an adhesion,

a diverticulum, a rent in the mesentery, a malformation of peritoneum, or a twisting of the tube," an operation may be undertaken because the obstruction, in its earlier stages, may be completely removed, and is incapable of spontaneous cure; but although an operation is justifiable it is not the exclusive remedy for this class of cases. Age, debility, previous or present peritonitis or inflammation of the obstructed bowel, would be strong contra-indications. If when the operation be undertaken a stricture be found instead of a band of lymph, it is proposed to establish a communication between the bowel above and below the obstruction, using a ligature of catgut or wire.

The object of all rational *medical treatment* in obstruction must be the protraction by every means in our power of those pathological processes which are its result. As we have before stated, Dr. Brinton proves that the fatality of obstruction depends directly, *cæteris paribus*, on the amount and rapidity of the distension, which is its necessary result. The steps that nature takes for overcoming the obstruction are, greatly increased peristalsis and consequent dilatation, and if these be properly moderated, and their injurious effects prevented, we may often fairly expect a favourable issue. In fact, the primary indication here, as in so many other instances which come under the notice of the physician, is to apply in the treatment that great principle of rest, which has been so successfully used in the treatment of surgical diseases, and the paramount importance of which has been so triumphantly asserted by Dr. Hilton. It is just as impossible for the natural processes of cure, in a case of intus-susception—adhesion, organization, and ulceration or sloughing—to pursue a favourable course where violent peristalsis and excessive distension tend to separate the segments of bowels as it is for a fracture to unite or a sprain to recover if the muscles of the injured part be not kept at perfect rest. Distension leads to paralysis of the portion of the bowel engaged, if it be rapid and excessive, while the same distension, when protracted and diminished in violence, may not only stave off death, but even mediate a complete recovery. Coaptation of the healthy segments of bowel adjoining the site of an intus-susception, are quite as necessary to its cure as is proximity of the broken ends of a bone to their union. Clinical experience furnishes another strong argument in favour of this principle of treatment, for it is almost invariably the chronic and protracted cases which recover.

"In accordance with the foregoing rational principle, the following

seem the chief indications of treatment:—to prevent distension; to assuage pain; to mitigate excessive peristalsis; and to support the patient's strength during what is necessarily an exhausting, and often a long illness."

It is proposed to attain the first and last of these objects by the following means:—

"The avoidance of distension is to be attempted by reducing, in every available way, the quantity of food and drink, restricting the latter (so far as the often excessive thirst of the patient will allow) to small but frequent sips of cool (or even iced) liquids, and administering the former (in the shape of strong beef-tea, soup, or milk) with almost equal frequency and caution. With such articles, we may usefully alternate small doses of alcohol; preferably in the form of brandy, with water or even soda-water. But inasmuch as the distension practically measures, not merely the danger, but the probable rapidity, of the case, the repugnance of the patient, or the instantaneous vomiting which these articles of food and stimulus often excite, must be met by a corresponding reduction in their doses. And it must often be a matter of great nicity to judge what is the proportionate urgency of these two antagonist suggestions for and against support, or how far one is to be subordinated to the other."

The mode of bringing about the other two objects—the alleviation of pain and the diminution of peristalsis, proposed by Dr. Brinton, is by no means novel: it was adopted at a very early period in the history of medicine by men who, although they confessed to its partial usefulness in many cases, yet they combined it in most cases with treatment which frequently nullified its good results. We find mention of the use of narcotics in these cases in the works of Aretaus Paulus, of Ægineta, &c.; and Sydenham especially dwells upon the usefulness of the liquid laudanum where purgatives could not be borne. The same principle suggested to Graves and Stokes the use of opium in enteritis and ulcer.

In selecting that narcotic which would be most suitable in most cases of obstruction, our author has proved himself to have been possessed of an intimate acquaintance with the physiological effects of drugs, without which we believe all his diagnostic skill and pathological research would have been comparatively useless when applied to treatment, and we regret that there are so many who excel in the former branches of medical science, yet fail in that branch of knowledge which has the most direct bearing on medicine considered as an art, and who even openly express their

scepticism as to the value of drugs in the alleviation or cure of disease. We consider that it is as little reproach to the physician that he cannot cure a cancer of the liver by drugs as it is to the surgeon that he is unable to bring back a gangrenous limb to life; that the latter may be removed while the former cannot is but a law of our existence which can never be altered. That man will best contribute to the elevation of medicine and its protection from quackery who, combining diagnostic skill and pathological knowledge with an intimate acquaintance with the nature and effects of medicines, labours long, and, it may be, anxiously to evolve some laws for treatment on the now well recognized principle of following the indications of nature, rather than in a vain search after specifics, in the existence of which few places much faith.

Following up views like these Dr. Brinton points out the superiority of opium (which he gave in the form of extract) over all the other narcotics, inasmuch as its stimulant properties and the comparative uniformity and regularity of its effects renders it peculiarly fit for administration in the course of cases which are so protracted and debilitating. One exception is made in favour of the use of a combination of belladonna and opium—namely, in the case of intus-susception, where it is found that opium, even when pushed to the verge of narcotism, does not effect any diminution in the violent straining peristalsis. His experience accords with our own with regard to the fact that the antidotal power of these drugs for each other does not reach to all the effects of either, but that in many cases the one acts as a useful *adjuvans* to the other. The proportion recommended by Dr. Brinton is two parts extract of opium, and $\frac{1}{2}$ to $\frac{2}{3}$ parts extract of belladonna in a pill.

The use of *tobacco* is confined to the earlier stages in the form of enema, where there is no sign of prostration, as its power of inducing collapse renders its general use most undesirable. *Enemata* are of use in all cases as a means of introducing nourishment or sedatives; in some by their warmth and moisture they act as an internal stupe, while, in a few cases, by their bulk, they may assist in removing the cause of the obstruction; they should always be administered by a skilled person, and are only useful where the patient has sufficient strength of mind to bear the consequent suffering, which is reduced to a minimum by the very gradual introduction of the fluid. The long tube has not, according to our author, any advantage over the short to compensate for the danger of its use.

Purgatives were used from time immemorial in these cases. It was reserved for Dr. Brinton to demonstrate their uselessness, and, in many cases, their fearfully injurious effects; for although various physicians had recognized their ill effects at a certain stage in the disease, to him alone belongs the credit of pointing out that "Nature herself did all that purgatives could do in any case, by preparing within the obstructed bowel a mass adapted by its quantity, quality, and consistence, to accomplish all that is possible for any aperient to do towards opening a passage."

On the contrary, their use is positively hurtful, not only at a late but also at an early stage, by distending the bowel so rapidly as to paralyse its coats, or to provoke an excessive and exhaustive peristalsis, or to excite a fatal degree of inflammation.

Yet we fear that there are still some who persist in the old path, and aggravate instead of curing the disease. Such a case came under our notice lately, and although the treatment was altered at a sufficiently early period to save the patient's life, it was at the expense of a very protracted convalescence. Calomel, crude mercury, inflation, counter irritants, are all considered by Dr. Brinton, and with the exception of inflation (of which he has had no experience) are condemned as worse than useless. Fomentations are occasionally of service, and a bandage may assist in relieving the throes of peristalsis.

No mention is made of the use of galvanism in the treatment of certain cases of obstruction, where even opium fails to give relief. We well remember a case which came under our notice, in which the obstruction was believed to be due to an impaction of feces, and the consequent paralysis so great as to resist all treatment. In this case galvanism was applied persistently (one pole at either side of the abdomen, or one to the back and the other to the abdomen) for several days, and was finally most successful. Dr. Stokes' case, which was published by Dr. Finny in the *Dublin Quarterly Journal* for November, 1864, will be fresh in the memory of our readers. In this case purgatives, enemata, and fomentations had been tried without success before Dr. Stokes saw the case. Subsequently opium, belladonna, calomel and opium, croton oil, strychnia, and colocynth, and galvanism externally applied, were attended with scarcely any effect, so that Dr. Stokes was led to propose as a *dernier ressort* the application of the galvanic stimulus to the mucous surface of the intestine. This treatment was at once crowned with success; "the abdominal muscles were thrown into

a state of violent spasm, and three large basinfuls of liquid feces were got rid of." The operation was attended with great agony, but no hard lumps passed at any time.

This mode of procedure is only applicable where the obstruction is due to paralysis, and would be most injurious in other forms of obstruction by inducing that violent peristalsis which it is our object to mitigate.

This interesting chapter is brought to a close by a summary of the treatment advocated in the several forms of obstruction. It is as follows:—

"In intus-susception of the large intestine, repeated injections of liquid into the rectum, so as to distend the bowel to its utmost dimensions.

"In stricture of the large intestine, the institution of an artificial anus above the obstacle.

"In obstruction from bands, diverticula, &c., mostly affecting the small intestine, gastrotomy, and division of the cord-like cause of strangulation; a procedure which, if interrupted by unforeseen impediments, may further require the institution of an artificial anus in the most distended part.

"In obstruction by stricture, however, a tobacco enema should be administered at least once; a measure which should be repeated, if need be, in the obstruction by bands, and especially by gall-stones.

"In all cases, opium and support to be freely administered from the earliest stage of the malady. The bulkier liquid constituent of the food to be given as sparingly as possible by the mouth, but administered freely per anum. Distensive enemata to precede all operations, if only as a means of aiding or assuring diagnosis. Where vomiting is excessive, nourishment to be also injected into the rectum in small and frequent doses.

"After recovery, all food which can introduce indigestible substances into the intestine should be carefully avoided; the bowel having sometimes undergone changes of calibre and arrangement such as permit substances easily transmissible through the healthy canal to cause fatal obstruction."

The volume is concluded with a note by the editor, Dr. Buzzard, detailing his part in the work owing to the lamented death of the author, and the notes of three cases successfully treated on the sedative plan, in one of which the patient took no less than ninety grains of opium.

We cannot conclude this notice without expressing our profound

regret at the loss sustained by the profession in the death of the author of this valuable work. His labours have tended to throw more light on diseases of the stomach and intestinal canal than those of any other individual, and the treatment which he has proposed in cancer and ulcer of the stomach, and in obstruction, has met with universal approval and cheering success.

Lectures on some of the Applications of Chemistry and Mechanics to Pathology and Therapeutics. By H. BENCE JONES, A.M., M.D., F.R S., late Physician to St. George's Hospital. 8vo, pp. 314. London: John Churchill & Sons.

A LONG career of able, original, and highly practical research, has placed the name of Dr. Bence Jones in the foremost rank of the cultivators of the sciences applied to medicine. In the work before us we find the result of his most recent inquiries, and we propose to pass in review the more important of his observations. The preface deals with the conservation and correlation of energy in the body. The constancy of force in the universe is a now generally recognized law in physical science; and in medicine each day's advance in inquiry shows how more and more clearly we are enabled to approximate in our explanations of "vital," that is, organic phenomena, be they chemical or mechanical, to the conditions which observation and experiment prove to be always obeyed in the phenomena of the physical world around us. "In plants and in animals," says Dr. B. Jones, "the same laws of the indestructibility, and the conservation of force, offer a new and vast field for experimental investigation into the relationships of the forces that are in action; and promise to unfold much, though possibly not all, of the mystery that is comprehended under the word 'life.'"

Further on he says—"But I shall endeavour to show, by a few striking examples, that the theory of the conservation of force opens a vast field of experimental inquiry, not only as regards the origin and connexion of the actions which constitute diseases, but also as regards the actions of the remedies by which those diseases are modified or removed."

"In proof of this I may take the first grand chemical action going on in the body—oxidation. How little of this do we know at present. Pettenkofer's experiments on respiration in health and disease will probably give us, for the first time, accurate information on the

amount of oxygen consumed by man in different circumstances. How does this oxidation take place? Whether the oxygen is made into the denser ozone (whose energy is to that of ordinary oxygen, as common phosphorus to the allotropic red phosphorus)? Whether Schönbein's views are correct—that every substance capable of being oxidized first makes the ordinary oxygen into ozone, and that this enters into combination, first slightly (still preserving its properties), and then firmly (when the ozone loses its characteristics)? Whether antozone exists, and by combining with ozone, forms ordinary oxygen? These are questions which show how much our knowledge is advancing, while they also show how much remains to be done before the chemistry of oxidation in the body can be thoroughly understood.

"One two-thousandth part of ozone in the air is said to cause dangerous engorgement of the lungs, and even smaller doses, long continued, cause bronchitis and pneumonia. The blood of animals killed by ozone, is found very rich in fibrin, and its dark colour shows that it has undergone active oxidation."

From the researches of Professor Stokes, that distinguished authority has been led to think that cruorine (hæmatoglobulin) takes no part directly in oxidation, but that it so exalts the energy of the oxygen that it enables it, in and out of the body, to do what it would not do in its ordinary condition. However, as remarked by Dr. Bence Jones, the most energetic oxidations occur outside the capillaries where no cruorine exists.

Amongst the diseases which may be recognized as errors of oxidation, the following are enumerated by Dr. Bence Jones:—Diabetes, acidity, the production of oxalic acid, uric acid, uric oxide, cystine, fatty degeneration, gout, rheumatism, inflammation; and among fermentations: eruptive fevers, continued fevers, intermittent fevers, small-pox, syphilis, pyemia, glanders, hydrophobia, plague. We are not prepared to go this length with our author; but the subject is still in too vague a position, as to the chemical aspect of all the phenomena concerned, to admit of any profitable discussion or critical consideration, with a view to a practical solution of the difficulties involved.

The following brief prefatory observations with regard to the role played by the nervous system in modern pathology are worthy of note:—

"We are just ceasing to regard the nervous force as the origin of all the powers in the body. Instead of making it a creator of force we

now regard it as the liberator and restrainer of the force in the body; in other words, as the regulator through the heart and blood-vessels of the extent and degree of oxidation and nutrition that occur in the human machine. Taking the various parts of the nervous system separately, there are—1st, Central organs; 2nd, conducting organs, transmitting impressions at the rate of twenty-eight or twenty-nine yards a second; 3rdly, sense organs; and 4thly, peripheral working organs; and it is in these that the regulating powers is made manifest. For example, mechanical, chemical, electrical irritation of the vagus stops the heart's action. Irritation of the sympathetic or division of the vagi increases the frequency of the heart's contraction. The ganglia of the heart seem to differ in their action. Experiments show that some have a liberating action—an automatic, rhythmic action; while other ganglia have a restraining action. Another example is seen in the vaso-motor nerves. Both Bernard and Schiff consider that these are of two kinds—the one set closing the vessels, and the other set of nerves opening them. When closed the blood is stopped; there is paleness and coldness, and no effusion of parenchymatous fluid: when opened, there is increased flow of blood, redness, and higher temperature, and increased exudation from the capillaries; in other words, the mechanical and thermal actions in the body are regulated by the nervous force acting on the heart and blood-vessels."

The experiments of Barral give us an approximate estimate of the different amounts of force lost (to the body) in different ways. From 1 to 2 per cent. of power is given out in the heat of the urine and feces; from 4 to 8 per cent. in the heat of the breath; from 20 to 30 per cent. in evaporation of water from the surface; and from 60 to 75 per cent. in conduction and radiation, and in mechanical work. Force enters the body as food, whether that be solid or fluid, animal or vegetable, æriform or mineral. It is converted into heat, muscular motion, the play of organs, its exercise of volition in all its phases; in fine, in all that characterizes the continuance of life. How does force finally leave the body? In the gradual restoration by *post mortem* chemical action of all its constituents to their ultimate simple elementary condition. What is it that controls and governs all force in the living body and liberates it in the dead? We know not; all that science, art, and observation have to tell us on this theme leaves us where we were centuries ago. Life is still the thing unknown, possibly unknowable.

Dr. Bence Jones's first chapter is devoted to the condition of "the chemical circulation," as distinct from the vascular. Of the

rapidity with which many substances are diffused through the tissues, observation has furnished us with many striking examples familiar to all. Madder diffuses rapidly into the bones, bile into all the tissues. Many substances run through the system with wonderful celerity; that ether, asparagus, and turpentine pass rapidly their essential odoriferous particles into the urine has been long well known. It is, however, to Dr. Bence Jones that we owe the institution of accurate scientific experiments, which prove in an incontestable manner the extraordinary rapidity with which quantities, the most minute, of certain metallic and other compounds can be found in the non-vascular tissues after their ingestion.

Both in plants and animals he has applied the well-known principles of spectrum analysis to the detection of substances artificially introduced. He states:—

"The delicacy of the spectrum analysis may be seen in this table, which gives the smallest quantity of each substance that can be thus detected:—

"Chlorate of soda,	$\frac{1}{105}$	millionth of a grain.
Carbonate of lithia,	$\frac{1}{8}$	,, ,,
Chloride of strontium,	1	,, ,,
Chloride of barium,	1	,, ,,
Chlorate of potass,	$\frac{1}{65}$	thousandth ,,
Chloride of lithium,	$\frac{1}{13}$	millionth ,,
,, rubidium,	$\frac{1}{16}$	thousandth ,,
,, casium,	$\frac{1}{125}$	,, ,,

Soda exists everywhere in and out of the body, and so was not examined for. Lithia was next sought. It appears to exist in potatoes seldom, apples sometimes, bread traces, cabbage distinctly, Rhine wines always, French wines distinctly, sherry and port distinctly, in tea and coffee slight traces, ale and porter slight traces, mutton, beef, and milk none. It had already been found in sea water, kelp, spring water sometimes, ashes of wood in the Odenwald, Russian and other potashes, tobacco, vine leaves and grapes, ashes of plants in the Palatinate, milk of animals eating the produce, ash of human blood and muscles, meteoric stones, all the drinking waters of London.

The following table gives the rate of the passage of the chloride of lithium from the stomach, not only into the circulation, but out of the circulation into the textures of a guinea-pig:—

"After 1½ grain was taken, in 3 days, plenty was found everywhere.

3	„	15 minutes,	everywhere	except in the lens.
3	„	30 „	„	„
3	„	30 „	„	traces in the lens.
3	„	30 „	„	outer part of lens.
3	„	60 „	„	„
3	„	60 „	„	except in the lens.
3	„	2¼ hours,	„	throughout lens.
3	„	4 „	„	„
3	„	8 „	„	„
3	„	24 „	„	„
3	„	26 „	„	„
¼	„	5¼ „	„	except in the lens."

It follows from these extracts, says Dr. Bence Jones, that three grains of chloride of lithium given on an empty stomach, may diffuse into the cartilage of the hip-joint, and into the aqueous humour of the eye in a quarter of an hour. In very young and very small pigs, the same quantity of lithium may, in 30 or 32 minutes, be found in the lens of the eye, but, in an old pig, in this time the lithium will have got no further than the humours of the eye. If the stomach was empty when the chloride of lithium was taken, then in one hour the lithium may be very evident in the outer part of the lens, and very faintly in the inner part; but if the stomach be full of food, the lithium does not in an hour reach the lens. Injected by the skin, the diffusion of lithium appears to be more active still. The rate of exit of the metal from the body is next discussed:—

"2	grains	in 6 hours	gave plenty everywhere;	in 6 days no trace in kidneys, liver, or lens.
2	„	„	„	„
2	„	„	„	in 4 days none in lens.
1	„	5	showed partly in the lens;	in 3 days faint in lens.

With the assistance of Mr. Bowman and Mr. Critchett, some interesting observations were conducted by our author on the diffusion of lithium into the tissues in the human subject:—

"Twenty grains of carbonate of lithia were taken twenty-five minutes before the operation for cataract; no trace of lithium was found in the cataract.

In $2\frac{1}{2}$ hours lithium was found in the watery extract of the cataract.
$3\frac{1}{2}$ „ „ in each particle.
4 „ „ „
$4\frac{1}{4}$ „ „ „
$4\frac{1}{2}$ „ „ „
5 (old man) „
5 „ „ „
7 „ traces in alcoholic extract of ash.
4 days in alcoholic extract, not the slightest trace of lithium.
7 „ slightest trace in the alcoholic extract.
7 „ „ „
7 „ „ „

In another experiment seven grains of carbonate of lithium were given eight hours before delivery, the lithium was detected in each particle of the umbilical cord. Thus it will be seen with what extreme rapidity this metal enters the system, and becomes diffused through its remotest parts and finest textures, while it requires days, five to seven, or even much longer, in some instances, to pass out of the body. The animal in part re-dosing itself by the re-absorption through the skin, of what has passed out in perspiration and in the hairs.

Of the highly interesting and important results in practical medicine which may be anticipated from the careful prosecution of observations such as the foregoing, we cannot but be hopeful.

Of the influence of an alkali in promoting chemical action, many examples are cited; oxygen does not act on pyrogallic acid until potass is added; sugar is reduced by the oxyen of the oxide of copper in the presence of alkali. An alkali splits fat into fatty acid and glycerine. Grape sugar exposed to ozone with potass, soda or carbonate of soda is entirely oxidized. It is highly probable that albumen is transformed into kreatin, uric acid, ozone, and other substances, only in the presence of carbonate and phosphate of soda in a temperature of 100 Fahr. Acids, on the contrary, passing by diffusion into the tissue neutralize the alkalies contained therein and thus stop oxidation. What light we may yet derive from an extension of these and similar observations to explain the phenomena of fevers it may be premature to inquire at present. We may, however, hazard the conjecture that in the excess of alkalinity in the blood and tissues may be sought one of the principal phenomena of fevers. With excess of alkali there must be excess of oxidation, that is excess of tissue change, excessive waste, excessive metamor-

phosis, and, as a consequence, excessive production of temperature. The administration of acids, especially the mineral acids, often hitherto empirically found useful, will gain an explanation based on scientific data, if these views be found tenable.

"The alkali," says Dr. Bence Jones, "disturbs the equilibrium of the elements in the organic body by its affinity for acids. Aided by oxygen and heat, more or less complex acids are formed from the neutral substances, and if the action of the alkali is sufficiently continued, carbonic acid, water, and ammonia, alone remain."

From the known action of the alkalies in the system, that of alkaloids may be assumed to exhibit a similar behaviour, differing, perhaps, in degree of intensity, though not in kind of operation. Our experimental knowledge of their powers of diffusion, and of their energy in the system is, however, as yet too limited.

"Thus," says Dr. Bence Jones, in his concluding observations on this subject, "the circulation of diffusion rises even to an equal, if not to a greater importance than that other more mechanical circulation of the blood, which, indeed, in two out of the four grand divisions of animals, is almost absent; and during the early weeks of our own fetal life is entirely wanting; and in this chemical circulation we recognize a link between the lowest vegetable, and the highest animal creation, since their diffusion is a necessary condition on which the chemical actions in both kingdoms of nature depend."

Amongst the most remarkable of recent discoveries in the chemistry of the human body, must be recorded that which has rewarded the unwearied spirit of research which so eminently characterizes Dr. Bence Jones. We allude to the existence in the textures of animals of a fluorescent substance closely resembling quinine, foreshadowed, it is true, to some extent, by the observations of Brücke, Helmholtz, and Regnauld. In conjunction with Dr. Dupré, Dr. Bence Jones was engaged in observations to determine the rate of passage of quinine into and out of the textures of animals, this alkaloid having been chosen in consequence of Professor Stokes's remarkable discovery of the change in the refrangibility of light due to the presence of quinine.

As exhibiting the delicacy of this test, the following results were arrived at, when the spark from a Ruhmkorff coil was the source of light:—

"Sulphate of quinine gave—

Slight fluorescence, when $\frac{1}{66000}$ of a grain was present.
Feeble " $\frac{1}{33000}$ " "
Distinct " $\frac{1}{25000}$ " "

"One grain of sulphate of quinine in one million eight hundred parts of water showed the blue fluorescence distinctly in twenty grains of the solution."

In following up their experiments on the passage of quinine through the textures of the guinea-pig, our authors were at first somewhat disappointed to find that fluorescence was observable both in those to which quinine had been administered, and those to which it had not. Attention was now turned to determine the fluorescent substance which apparently resided naturally in the textures, and it was ascertained, that without any previous preparation, it can be proved to exist in the living, and in the dead textures. It can be demonstrated in the recent lens after death, and can be equally well shown to exist in the human lens during life. To this substance the name of Animal Quinoidine has been applied, as in all respects its behaviour with the ordinary re-agents is similar to, if not identical with that of the well-known alkaloid quinine. The quantity of the fluorescent substance is approximately shown by the following table, exhibiting its amount in grains in different parts of the guinea-pig and in man, measured by the number of grains of quinine in 100 litres (= 176 pints) of water, that gave the same fluorescence:—

	In Guinea-Pigs			In Man		
Liver, - - -	6–3	6	2	2	2	2
Lenses, - - -	3	2	2	2	—	—
Kidney, - - -	3	2	2	2	2	3 to 6
Urine, - - -	3	2	2	—	—	—
Bile, - - -	3	2	2	—	—	—
Blood, - - -	3	2	2	—	—	—
Brain, - - -	3	2	2	—	—	—
Nerves, - - -	2	2	2	—	—	—
Muscles, - - -	3	2	2	3	1	2
Humour of the Eye, -	2	2	2	1	2	2
Cartilages, - -	—	—	—	3	1	—
Spleen, - - - -	—	—	—	1	1	3
Lungs, - - -	—	—	—	1	1	2

With regard to the origin of the Quinoidine, Dr. B. Jones looks upon it as a derivation from albumen; and he furnishes us with two parallel columns exhibiting the synthetic products formed by the plant or the chemist, starting, on the one hand, from carbonate of ammonia and water, and culminating in albumen, and on the other the substances formed analytically from albumen, in its passage downwards, to carbonate of ammonia and water, animal quinoidine taking its place next to caseine as third in order.

Thus we see how the cinchona plant and the animal alike may form quinine from such simple elements as carbonate of ammonia and water.

Sulphate of quinine is shown to pass with great rapidity into the textures. In a quarter of an hour after four grains of sulphate of quinine were taken the fluorescence was found to rise to 75 grains to 100 litres of water. It is found in the greatest amount in the liver and kidneys; rather less in the blood, urine, and muscles; still less in the brain, nerves, and bile. In three hours the maximum effect of the quinine appears to be reached. In six hours the fluorescence was less than in three; and in seventy-two hours it had disappeared altogether. The nerves would seem to show less of the fluorescent property after ingestion of quinine, than the other parts named. This is a result not a little remarkable, and certainly different from what we might be led *a priori* to anticipate, having in view the effects upon the nervous system ordinarily attributed to the action of quinine.

In an elaborate table the fluorescence of the different textures is shown after experiments in which four grains of sulphate of quinine had been administered to guinea-pigs.

As bearing on the action of quinine in the system, the following passage explaining the views of so accurate an observer as our author are worthy of being cited:—

"Thus then quinine goes everywhere; and wherever it goes it meets with the natural fluorescent substance like quinine, which most probably is constantly forming and undergoing oxidation. The incoming quinine causes a temporary excess of quinine in the textures. Probably it causes a stoppage of the fresh formation of quinine from albumen; a temporary arrest of the changes going on; a transfer of action perhaps to the quinine introduced, so that, with large doses, deafness and great prostration, and almost imperceptible pulse, are produced in man, whilst in guinea-pigs death even is caused by the extreme prostration. In small doses, quinine, probably like alcohol, gives an immediate stimulus when the first chemical action takes place; but soon the quinine retards the chemical changes in

the nitrogenous substances, just as alcohol by its secondary action retards the chemical changes in the hydro-carbons of the different textures."

Of the high value and even practical importance of these views it is impossible to entertain a second opinion, and it is undoubtedly from the prosecution of researches such as those we have just described, and upon principles like those just enumerated, that we may look forward to the establishment of a rational system of therapeutics in that future so rich in promise for the physician who shall put himself in a position to render available, for his art, all that is furnished to his hand by researches and advances in the sciences allied to medicine. We have dwelt thus at length and in detail on the earlier chapters of Dr. Bence Jones's work, because of the importance and the novelty of his observations. This portion of the work would constitute, in fact, a separate treatise of much value if fully developed in all the expansion and detail its subject matter admits of, and to no hands could such a work be better committed than to those of our author.

In the chapters which follow, we find the clinical application of the principles discussed in the preliminary section to the diagnosis and treatment of disease. Lecture III. is devoted to Diabetes. We have first a very full exposition of the author's views on this still obscure and important malady, with some interesting cases dealt with from an original point of view. It is still to be admitted that no known remedy is capable of controlling this singular affection, which from all we have now pretty well established as to the nature and order of succession of its chief phenomena, must be regarded as a lesion of function. The effect of diet is far beyond that of any drug which the pharmaceutist has yet supplied us with. An anti-farinaceous, or in other words, an anti-saccharine diet, will remove the sugar from the urine, and stop all the symptoms of the complaint in all those cases in which the power of causing the animal sugar remains unaffected.

Even when the consumption of the animal sugar is imperfect, or impossible, an anti-saccharine diet will lessen the thirst, the flow of water, the dryness of the mouth, and even the constipation, and check, though it may not stop, the waste.

The simplest formulæ for the diet may be thus stated:—"All animal produce, including fish, flesh, fowl, game, eggs, cream, and meat soup, should be taken; and all vegetable food that contains starch, dextrin, and sugar, should be avoided."

Very particular and explicit details next follow, which show the relative values, as sugar-suppliers, of various kinds of vegetable food, and of various wines and liquors; and at page 62, and five following pages, will be found *in extenso*, an enumeration of the dishes suitable for diabetic patients, according to the principles laid down by M. Bouchardat, and which Dr. Bence Jones says very frankly, "Is as much superior to any table I could draw up, as French is to English cookery." The publication of this table in so accessible a form, cannot but be of great practical service to the physician in charge of diabetic patients.

The ensuing chapters are devoted to "Acidity," "Gout," "The Oxalic," and "The Xanthin Diathesis," "The Cystin Diathesis," "The effects of Cold," on "Fatty Production," "Inflammation," "Bright's Disease," on "Fermentations and Fevers," on "Subtrophic and Pertrophic Actions," on "Surgical and Medical Diseases," and on the "Actions of Medicines." In each and all of these sections will be found matter of high interest, illustrated by original inquiry, and with instructive bearing on practice. And we can only regret that our limits prevent us from following in detail the footsteps of our author. But we have said enough of the contents of this valuable volume to ensure its warm reception at the hands of all those who aim at substituting a rational practice of their profession for a blind empiricism.

Neligan's Medicines: Their Uses and Mode of Administration. By RAWDON MACNAMARA, Licentiate of the Royal College of Physicians; Licentiate, Fellow, Member of Council, and Professor of Materia Medica, Royal College of Surgeons in Ireland; Surgeon to the Meath Hospital, &c., &c., &c. Seventh Edition. Dublin: Fannin & Co., Grafton-street. 1868. Pp. 934.

THIS is a book, of which not only the author and publisher, but even the medical profession in Ireland, have every reason to feel proud. A work of the kind, more concisely, or more carefully written, or one containing more information, has not as yet come under our observation. Very few works attain to the honour of a second edition, and perhaps not one per cent. reach a third issue. Of the five thousand works to which the prolific press of Great Britain gave birth in the year 1867, not one in three hundred was a fourth

edition. It is true, that works pertaining to certain departments of literature, whilst destitute of intrinsic value, may, in a short time, pass through half a score of editions; yet, it may safely be asserted that no scientific manual not possessed of great merit, ever becomes a standard work. The best evidence of the high estimation in which *Neligan's Treatise* is held by medical practitioners and students, is afforded by the simple fact of its having attained to a seventh edition within a period of about twenty years. Every reader of such a work is, more or less, a critic; and when we find many thousands of critical readers purchasing, year after year, copies of *Neligan's Materia Medica*, we may fairly conclude that the professional opinion of its character is in the highest degree favourable.

The merit of the plan of this treatise is due to the late Dr. Neligan; and in his hands the work passed through five editions, the last of which included 580 pages. In 1864 the publication of the *British Pharmacopœia* rendered the current edition of Neligan's *Medicines*—at that time, however, out of print—almost useless; but for some time previously the preparations for a new edition had been commenced. At that time Dr. Neligan's failing health precluded him from engaging in any kind of literary labour, and the execution of the new edition was entrusted to Dr. Rawdon Macnamara, who completely re-cast the subject matter, and added 150 pages to the volume. Last year a second and corrected edition of the *Pharmacopœia* was issued; and this circumstance, and, as we are informed, the complete exhaustion of the sixth edition, account for the appearance of the present handsome volume of 934 pages, which is, in almost every respect, a perfectly new book, as compared with the fifth edition. Indeed, we recognize so little of Dr. Neligan's labours in it, that in our opinion it would have been fairer to both the original author and the present editor, had the publishers induced the latter to assume the whole responsibility of the undertaking; to bear all the blame of any errors that it might be found to contain; to enjoy all the credit to which it might be considered as entitled. In a future edition we hope to see these suggestions acted upon by the excellent and enterprising publishers.

This work, like many others of the same kind, is, to a great extent, based upon the *Pharmacopœia*, but it supplements the matter contained in that official volume with a vast amount of information —medical, chemical, and botanical. Indeed, excepting the diagnosis of disease, there is scarcely a department of the theory and practice of medicine on which it does not treat. The work is divided into

twenty-two chapters, and contains three appendices. The contents are distributed in these divisions in the following manner:—In each chapter are given all the medicines which are connected with each other by one or more strong points of resemblance in their therapeutical action; for example, Chapter I. is wholly occupied with the consideration of ANTACIDS, under which head is included all the preparations of soda, potash, ammonia, lithia, and the earths that exhibit an alkaline reaction. We have the subjects—manna, honey, senna, jalap, aloes, calomel, &c., discussed under the head of CATHARTICS; vinegar, creasote, sulphate of copper, &c., in the chapter on Astringents; opium, belladonna, henbane, under the class of NARCOTICS. In this way twenty of the chapters are occupied, the last two being respectively devoted to WATERS and SUPPLEMENTAL AGENTS. This is a very excellent methodical arrangement, and is open to but a single objection, namely, that some of the articles described under one head, must be also considered under another. For example, the preparation, chemical and physical properties, adulterations, and general therapeutical properties of magnesia, and its alkaline compounds, are described under the head of ANTACIDS; but some of these bodies are again discussed, very briefly, however, in the chapter on CATHARTICS. This procedure causes a waste of space, and sometimes leads to repetitions; for example, we are informed in the first article on magnesia (page 211), that "practitioners in prescribing it, should bear in mind the danger, after long continued use, of its forming concretions in the alimentary canal, depending, as it does, for its elimination on the amount of acids which it may meet in the primæ viæ. Many cases are on record where large masses have been met with in the intestines, agglutinated together with intestinal mucus, months after its administration had been discontinued." This information is again, in substance, given in page 182, under the head of CATHARTICS. Sulphate of zinc figures under four different heads, and the same may be said of many other articles of the *Materia Medica.* We repeat, this is a drawback to the systematic mode of arranging the contents adopted, but it is one which we do not think admits of a remedy. Had the medicines been grouped under the heads of the various diseases, in the treatment of which they are employed, such a method would have been open to the grave objection of encouraging empiricism. A book so arranged would, no doubt, be largely consulted by quacks —*with or without medical qualification*—and might be looked upon as a kind of substitute for *Buchan's Family Medicines.* We believe,

therefore, that although the distribution of some of the medicines described in this book under different headings, is sometimes inconvenient, and renders the descriptions incomplete, or rather disjointed, it is the best one that could be adopted.

Each chapter opens with a concise general description of the class of remedies on which it treats; then each article is described in alphabetical order. If it be a chemical substance and an officinal one, its preparation, according to the *Pharmacopœia* directions, is given, and the changes that take place in its formation are explained, the use of chemical symbols and algebraic formulæ being employed. We notice that the formulæ of the chemical substances are given according to both the old and the new theories. In this matter the author follows the example shown by the editors of the *Pharmacopœia*. In explaining, however, the decompositions and other changes that take place in the various processes given in the *Pharmacopœia*, the author adheres to the old system of notation. It is to be regretted that he did not employ both methods, as the new one is now all but generally adopted in these countries, and many students are not acquainted with the old one. It is probable that Dr. Macnamara's reason for not using both systems in these cases, is the great economy of space —an important point when a volume so bulky is concerned—which he effects by employing one only. We trust, however, that by the time an eighth edition is called for, the practitioners and students of medicine will be prepared to discard the old system altogether.

The preparation of the substances having been gone into, its physical attributes and chemical history are detailed so fully and accurately as to leave nothing in connexion with these subjects to be desired. If the article is liable to the sophisticator's art, the various kinds of adulteration are indicated, and the processes by which these serious frauds may be detected, fully explained. Next in order comes the most important of the divisions into which the description of each article is distributed, namely, its therapeutical effects. This subject, though of the greatest utility to the student, will be most appreciated by the practising physician. In treating upon it, the author displays his erudition, his critical powers, and his total want of prejudice. The number of authorities quoted under this head is extremely great, but the information which the author has gained from his extensive reading is not presented to us in a crude state. It is served up to us in remarkably clear, concise language; and although he quotes from the writings of several thousand physicians, he does not, except in two instances,

give a single extract from any paper, essay, or book. This is, to our mind, a meritorious feature in this work, and one in which it contrasts favourably with many of the medical books lately published, in which three-fourths of the subject-matter are contained within inverted commas. The different officinal preparations of the article —such as "Solution," "Liquor," "Powder"—are stated, the dose of each mentioned, and the "mode of administration" described. The style in which the book is written, and the method of arrangement, are well exemplified in the following extract, which we select on account of its completeness and convenient length:—

"*Iodum.*—*Iodine.*—(A non-metallic element obtained principally from the ashes of sea-weeds.) I (=127). This elementary substance was first described in 1811 by M. Courtois, of Montpelier, who recognized its presence in kelp, in which it exists in combination with potassium, sodium, and magnesium.

"*Preparation.*—Iodine is an article of the Materia Medica in the British Pharmacopœia. It is procured by the manufacturers on the large scale from the ashes obtained by carefully burning various species of sea-weed. These ashes, technically called *kelp*, are washed with water, to which they yield about half their weight of salts. The mother liquor is poured off from the salts (sulphate and carbonate of soda and chloride of potassium), which are deposited by evaporation and crystallization; it is then treated with oil of vitrol, which sets free carbonic acid, sulphurous acid, sulphide of hydrogen gas, and sulphur, which latter is precipitated, whilst the iodine is converted into hydriodic acid. To the mother liquor peroxide of manganese is added, which decomposes the hydriodic acid, its oxygen uniting with the hydrogen to form water, setting free the iodine, which is recovered by sublimation, whilst the resulting protoxide of manganese unites with the sulphuric acid to form sulphate of manganese, thus, $HI+MnO_2+SO_3=HO+I+MnOSO_3$. M. Barruel has suggested this modification of the process. After as much of the salts as possible are gotten rid of by crystallization, the mother liquor is to be evaporated to dryness, and the resulting mass ignited with peroxide of manganese, by which all the sulphides, sulphites, and hyposulphites are converted into sulphates. The residuum, dissolved in water, is to be treated with a mixture of two parts and a half of sulphate of iron, and one part of sulphate of copper, so as to precipitate the subiodide of copper; this is heated with binoxide of manganese and sulphuric acid when the iodine is disengaged in violet vapours, which condense into black crystals as they cool. Iodine as it occurs in commerce being, however, seldom sufficiently pure for medical purposes, should be purified as follows:—Take of iodine of commerce, one ounce. Introduce the

commercial iodine into a porcelain capsule of a circular shape, cover this as accurately as possible with a glass matrass filled with cold water, and apply to the capsule the heat of boiling water for twenty minutes. Let the matrass be now removed, and should colourless acicular prisms of a pungent odour be found attached to its bottom, let them be separated from it. This being done the matrass is to be restored to its previous position, and a gentle and steady heat (that of a gas lamp answers well) applied, so as to sublime the whole of the iodine. Upon now allowing the capsule to cool, and lifting off the matrass, the purified product will be found attached to the bottom of the latter. When separated it should be immediately enclosed in a bottle furnished with an accurately ground stopper.

"*Physical Properties.*—Iodine is generally met with in the form of small crystalline scales, often accreted into masses of a bluish-black colour with a metallic lustre. It has a strong disagreeable odour resembling that of chlorine, and a very acrid taste. From a solution in liquid hydriodic acid, it may be obtained in tolerably large crystals, which are oblique octohedrons with a rhombic base. Its density is 4·948.

"*Chemical Properties.*—Iodine is an elementary body existing in combination in both kingdoms of nature; its equivalent is 127. It evaporates slowly at the usual temperature if exposed to the air, and more rapidly if moistened; fuses at 224° and boils at 356°. Exposed to an increased temperature it is volatilized in the form of a beautiful violet-coloured vapour, from whence it has derived its name (Ἰώδης, violet). Iodine requires 7,000 parts of pure water for its solution, to which it imparts a brownish colour; is much more soluble in alcohol, and very soluble in ether. The presence of tannin in water renders iodine more soluble in that liquid, which property may be taken advantage of in prescribing it in medicine, the addition of any astringent tincture or syrup increasing its solubility. Solutions of the iodides in water dissolve much iodine. The best characteristic of iodine is its action on starch, forming with it an *iodide of starch*, blue in colour; so delicate is this test that it will detect one grain of iodine in a million grains of water; a temperature of 160°, however, destroys this colour, a fact which should be borne in mind when applying this test.

"*Character and Tests.*—In laminar crystals, of a peculiar odour, dark colour, and metallic lustre, which, when heated, yield a beautiful violet-coloured vapour; very sparingly soluble in water, but freely dissolved by alcohol, by ether, and by a solution of iodide of potassium. The aqueous solution strikes a deep blue colour with starch. It sublimes without leaving any residue, and the portion that first comes over does not include any slender colourless prisms emitting a pungent odour. 12·7 grains dissolved in an ounce of water containing fifteen grains of iodide of potassium require for complete discoloration 1,000 grain-measures of the volumetric solution of hyposulphite of soda.

"*Adulterations.*—Iodine is frequently adulterated with fixed substances, such as charcoal, plumbago, black oxide of manganese, &c., all of which may be readily detected by their not being sublimed on the application of heat, or by their being left as an insoluble residue when iodine is treated with alcohol. Attention has been also directed by Professor Christison to an adulteration of much consequence, that with water, of which it frequently contains from 15 to 20 per cent.: that is to say, ʒj. of iodine may contain gr. xc. or even more of water. It may be readily detected by pressing a specimen between folds of filtering paper, or by shaking it in a very dry bottle. If greater accuracy be required, the volumetric test of the Pharmacopœia may be applied, which admits of no impurity, and which will be understood by reference to what has been already written, page 552. The slender pungent crystals alluded to in the pharmacopœial tests are *iodide of cyanogen*, an occasional impurity sometimes present to the extent of one per cent., and to which attention was first directed by Meyer and Klobach.

"*Therapeutical Effects.*—Introduced into the stomach, iodine exerts a local irritant action on that viscus, causing nausea and vomiting; in large doses, it produces the effects of an irritant poison; but in many instances, even when taken in enormous quantities, it has caused scarcely any effect if dissolved in a large quantity of fluid. In slight or medicinal doses, iodine acts as a special stimulant to the glandular system, generally affecting at the same time the organs of secretion, stimulating them to increased action. Under the continued use of small doses of this medicine, the removal or palliation of disease will sometimes take place without any perceptible action on the system generally; in other instances much emaciation and derangement of the digestive functions will be produced; while the very reverse effect, namely, deposition of fat and increased appetite, is very frequently observed as the consequence of a lengthened administration of iodine. So far as I have observed, the deposition of fat is consequent on its administration in small doses; the absorbents are thus stimulated to *moderately* increased action, whereby food is more thoroughly assimilated, and the individual grows fat; in large doses the action of the stomach is interfered with, the appetite more or less impaired, but the action of the absorbents, intensified by the iodine, exhibits itself on the adopose tissue already stored up, and consequently the individual grows thin. Recently an extract of the *fucus vesiculosus* in two to five grain doses three times a day has been suggested for the purpose of reducing obesity, for which purpose it is stated to be very effectual. I entertain no doubt that any property in this respect that it may possess is due to the iodine it contains. A curious statement, the truth of which I am inclined to doubt, for I cannot discover any authentic record of its having been witnessed in this country, was put forward some years ago on the Continent; that absorption of the mammæ in females and

wasting of the testicles in males have been produced by the continued administration of iodine. A remarkable train of symptoms, characterizing a peculiar disordered state of the system which has been named *iodism*, occasionally arises when the use of iodine in frequent small doses has been persisted in for a long time. These symptoms are nausea, headache, general langour and loss of appetite, followed by vomiting and purging, extreme depression, frequent small pulse, great weakness, fainting, and dry cough, occasionally attended with inflammation of the mucous membrane lining the air-passages, and terminating in death, if the use of the iodine be not abandoned in time. Iodism is, however, in the present day rarely witnessed, and when it does occur, is easily checked by suspending the use of the medicine. A far more common idiosyncrasy in connexion with iodine is the development in some individuals of symptoms resembling coryza. I know two or three persons in whom the smallest dose either of iodine or of iodide of potassium will immediately develop this most remarkable train of symptoms. Iodine is a most valuable remedial agent in the treatment of glandular enlargements, and in scrofulous affections; but its employment is contra-indicated when acute inflammation is present. In bronchocele it has proved more successful than any other remedy; indeed there are few cases, unless where the thyroid gland has become completely indurated, which will withstand the use of iodine when continued steadily for six weeks or two months; and even cases where the gland is much indurated are often remarkably relieved. In the innumerable varieties of scrofulous affections this remedy is most extensively employed and with decided advantage. It is found particularly beneficial in glandular swellings, tumours, abscesses, ulcers, ophthalmia, and diseases of the bones occurring in scrofulous constitutions. Iodine has also proved eminently successful in chronic enlargements of the abdominal viscera, particularly of the liver, spleen, and ovaries. There are no remedies which in my experience prove so successful in the treatment of cutaneous diseases, especially those of a chronic character, as iodine and its preparations; but to prove beneficial, their administration must be persisted in for some time, until in fact the system is manifestly brought under their influence. In fine, iodine has been proposed as a remedy in phthisis, in amenorrhœa, in leucorrhœa, in gout, in palsy, in chorea, in ascites, &c.; but in all these cases its success is very equivocal. The inhalation of the vapour of iodine was at one time very much used in the treatment of phthisis, and of chronic bronchitis, but general experience has proved its inutility. An injection, originally suggested by Sir Ranald Martin, composed of one part of tincture of iodine and three parts of water, is used after tapping, with most successful results, for the radical cure of hydrocele. Topically, iodine is employed in the form of tincture, of ointment, or of a solution in water, as a local stimulant in many forms of chronic cutaneous diseases, to

enlarged glands, in chronic swellings of the joints, to inflamed bursæ, to buboes, over large chronic abscesses, in erysipelas, &c.; but its external employment requires caution, as, if employed in too concentrated a form, it is apt to excite severe local inflammation. A remarkable case of this kind has been placed on record by me in vol. 40 of the *Dublin Medical Press*, p. 65. No matter in which of these ways iodine be employed, whether internally, or as an injection in hydrocele, or painted on the surface for any length of time, it is absorbed, and can be detected by chemical reagents in the various secretions. The urine is that generally selected for this purpose; and inasmuch as the iodine, although introduced in the free state, becomes combined with some base in its transit through the system, it is essential, previous to applying the starch test, to set it free, which is generally done by nitric acid. Presuming the base with which it has associated itself to be sodium, three equivalents of the iodide will require four equivalents of nitric acid, one of which is resolved into nitric oxide gas, which escapes, and three atoms of oxygen, which unite with the three sodiums to form three sodas, which uniting with the remaining three nitric acids form three nitrates of soda, whilst the three iodines unite with three equivalents of starch to form three iodides of starch; thus, $3NaI + 4NO_5 + 3Am = NO_2 + 3NaONO_5 + 3AmI$. After using it as an injection for the radical cure of hydrocele, I have thus detected it within five minutes in the urine of the patient. In poisoning with iodine, emetics should be administered, and their operation aided by the use of demulcent and amylaceous drinks, as starch, flour, &c., diffused through tepid water or milk.

"*Dose and Mode of Administration.*—Iodine is not administered in substance; and as it is usually given in combination with iodide of potassium, there are no simple preparations of it contained in the Pharmacopœia. As a rule, iodine and its preparations should be administered *before* meals, when we wish to produce decidedly its specific effects.

"*Preparations of Iodine*—Cadmii Iodidum (see p. 624); Ferri Iodidum (see *Tonics*); Hydrargyri Iodidum Rubrum (see p. 645); Hydrargyri Iodidum Viride (see p. 647); Linimentum Iodi; Linimentum Potassii Iodidi cum Sapone (see p. 686); Liquor Iodi; Pilula Ferri Iodidi (see *Tonics*); Potassii Iodidum (see p. 683); Sulphuris Iodidum (see p. 687); Syrupus Ferri Iodidi (see *Tonics*); Tinctura Iodi; Unguentum Cadmii Iodidi (see p. 626); Unguentum Iodi; Unguentum Plumbi Iodidi (see p. 680); Unguentum Sulphuris Iodidi (see p. 688); Vapor Iodi.

"*Linimentum Iodi.—Liniment of Iodine.*—(Take of iodine, one ounce and a quarter; iodide of potassium, half an ounce; camphor, a quarter of an ounce; rectified spirit, ten fluid ounces. Dissolve the iodine, iodide of potassium and camphor in the spirit.) This liniment is but half the strength of the Linimentum Iodi, 1864; the camphor is now introduced into it for the first time. This preparation is intended solely for

external use; it should be applied cautiously, as it is an energetic vesicant, to produce which effect it is well suited.

"*Liquor Iodi.—Solution of Iodine.*—(Take of iodine, twenty grains; iodide of potassium, thirty grains; distilled water, one fluid ounce. Dissolve.) Another of the uncalled-for formulas in the Pharmacopœia, one which might well have been kept for extemporaneous prescription. It may be used as a substitute for the tincture for external use, or well diluted with water it may be used for the preparation of solutions such as those of Lugol, decribed below; but even the pharmacopœial authorities seem at a loss to know what to do with this production of theirs, as they ascribe to it no dose, nor otherwise indicate its proposed use.

"*Tinctura Iodi.—Tincture of Iodine.*—(Take of iodine, half an ounce; iodide of potassium, a quarter of an ounce; rectified spirit, one pint. Dissolve the iodine and iodide of potassium in the spirit.) This may be used as a paint, over enlarged glands, &c.; it may be applied for a long time before it will vesicate; it may be also administered internally in ten to twenty minim doses; it is miscible with water, and is used in the preparation of the Vapor Iodi.

"*Unguentum Iodi Compositum.—Compound Ointment of Iodine.*—Take of iodine, thirty-two grains; iodide of potassium, thirty-two grains; proof spirit, one fluid drachm; prepared lard, two ounces. Rub the iodine and the iodide of potassium well together, with the spirit, in a glass or porcelain mortar; add the lard gradually, and mix thoroughly.) A convenient form for embrocation over enlarged glands, with a view to their dispersion.

"*Vapor Iodi.—Inhalation of Iodine.*—(Take of tincture of iodine, one fluid drachm; water, one fluid ounce. Mix in a suitable apparatus, and, having applied a gentle heat, let the vapour that arises be inhaled.) Another new and most uncalled-for preparation, one which will always require extemporaneous prescription.

"*Ioduretted Mineral Waters*, Lugol.—(These solutions are of three strengths—No. 1 containing gr. $\frac{3}{4}$ of iodine and gr. $j\frac{1}{2}$ of iodide of potassium; No. 2 gr. j. of iodine and gr. ij. of iodide of potassium; No. 3, gr. $j\frac{1}{4}$ of iodine, gr. $ij\frac{1}{2}$ of iodide of potassium; dissolved respectively in eight ounces of distilled water.) These three solutions are of a convenient and useful strength for the employment of iodine. Lugol's plan for using them was to commence the treatment with six ounces daily of No. 1, which was to be persevered with for two weeks, when the entire quantity was to be consumed daily for a week or so longer: the patient was then to proceed to No. 2, and consume the entire quantity each day, and finally to complete the cure with No. 3.

"*Incompatibles.*—Ammonia, sulphur, phosphorus, metals and their salts, hydrosulphates, sulphuric, nitric, and hydrocyanic acids, and the vegetable alkaloids."

In the case of substances of vegetable origin, the plants from which they are derived are pretty fully described; their chief botanical characteristics are stated, and the natural family, and Linnæn class and order to which they belong, are mentioned. For the use of students an appendix is given, containing a classified arrangement of the principal plants used in medicine, together with the preparations of those of them that are officinal The arrangement is according to the natural system. This table, the work of Dr. Hewett, is a very valuable one, and will be found of great use to the student.

We have read very carefully through this book, and, large as it is, we have found but very few errors of omission or commission, and such as exist, are of so very trivial a character, that they do not, in the slightest degree, detract from the value of the work. In the article on opium, where so many analyses of that substance are given, and the labours of so many chemists, British and foreign, acknowledged, we are somewhat surprised that no reference is made to Dr. Thomas Anderson's researches: they are both recent and important. In referring to works on Entozoa, it would have been well to have mentioned Dr. Cobbold's splendid work, more especially as it is, unlike most works of the kind, a production of the British press. In the article on Soda Water, it is stated that that popular beverage often consists solely of a solution of carbonic acid gas in water. Our experience of that fluid is, that in nineteen instances out of twenty, it contains not only soda, but also common salt; and speaking of soda water leads us to remark, that a chapter of this book is, for the first time, wholly devoted to the subject of "Waters." In this section, which includes 24 pages, a brief description (sufficient, however, for all, save the hydropaths), of natural, distilled, and mineral waters is given. The information contained in this chapter, will be found extremely useful to those whose limited libraries do not contain any special works on mineral waters. The analyses given of mineral waters are, as a rule, those most recently performed, and by the most eminent chemists. We are glad to find noticed the excellent sulphurated and chalybeate springs of Lisdoonvarna, in the county of Clare. The composition of these waters show that they are equal, if not superior, to most of the springs of the same class in England, and on the Continent. The sulphurated spring of Strathpeffer, Scotland, of which Dr. Macnamara says, that "from an examination of its chemical ingredients, one should imagine it to be possessed of valuable remedial powers," is to our mind, not only greatly inferior to the Irish spring, but positively dangerous. In

the water at Lisdoonvarna, there is no sulphate of calcium (nor *chloride of lime* as quoted) present; whereas, in the Scotch spring, the amount of that objectionable ingredient is no less than 39·45 grains per gallon. There is every reason to believe that water containing more than twelve grains of sulphate of calcium is likely to give rise to various affections of the alimentary canal, and to induce attacks of cholera, when that disease is epidemic. According to Parkes, any amount of sulphate of calcium, beyond three grains per gallon, is to be looked upon as excessive and unwholesome proportions; and we believe that the best springs are those in which the sulphate of calcium exists but sparingly.

Another new feature of this edition is a special chapter on "The Administration of Medicines," mention of which is omitted from the table of contents, being, no doubt, a printer's blunder. In this section we have very ably discussed the subject of "prescriptions." The author believes that whilst the modern physicians have wisely abandoned the absurd practice prevalent in the last century, of ordering forty or fifty different substances in a single mixture, they are in danger of drifting into the opposite extreme, an observation which coincides fully with our own views on the subject. Medicines may, in this respect, be compared with foods, for whilst the daily use of a great variety of the latter is usually followed by sad results to the animal economy, on the other hand, too much simplicity in diet is nearly as great an evil. There is no doubt but that, as a general rule, three or four ingredients should, at the least, be ordered in a prescription. Dr. Macnamara thinks so, and he divides the constituents of a mixture into—1st, the *remedium*, or special agent, in fact, *the* remedy. 2nd, An agent, the *corrigens*, the presence of which is intended to correct any unpleasant property possessed by the *remedium*. 3rd, The *adjuvans*, the therapeutic action of which resembles the remedium, and, therefore, supplements its effects; and 4th, The *menstruum* or *vehiculum*, which forms the bulk, and least active portion of the medicine. It is in the scientific arrangement of these constituents of a mixture that the skill of the prescriber is really shown, and as an example of what may be termed a scientific formulæ, the author gives us the following prescription:—

Remedium	Sulphatis magnesiæ, ʒi.
Corrigens	Syrupi zingiberis f ʒss.
Adjuvans	Tincturæ sennæ, f ʒi.
Menstruum	Infusi sennæ, ad ʒviii.

Here the action of the remedium is not only assisted by the *adjuvans*, but this latter assists the *corrigens* in its action, in virtue of the spirit and aromatics which it contains; whilst the *menstruum* not only acts as such, but also acts as an *adjuvans*, and in a minor degree, in virtue of the ginger which it contains as a *corrigens*. The corrigens again discharges a double duty.

The subjects treated upon in this chapter will be found amongst the most interesting, as they are certainly the most original in this work.

We should not omit to state that, although by far the largest portion of this work is devoted to the chemistry and botany of the Materia Medica, yet, the information with reference to the therapeutical and physiological actions is very ample, more especially in the cases of the more important articles. We select, as an illustration of the style of this department of the work, the excellent account of chloroform:—

"*Therapeutical Effects.*—From the time of its original discovery, *exhibited internally* in the fluid form as a sedative, chloroform has been more or less used on the continent and in America, but was very little employed in this country. The chief diseases in which it has been administered with benefit are asthma, spasmodic cough, and cancerous and other painful affections; in cancer it was most highly praised by Mr. Tuson, of London, but general experience has not confirmed his extravagant statements. More lately it has been given with excellent effect in obstinate vomiting, in painful affections of the digestive organs, especially the various forms of colic, and in nervous and spasmodic diseases, such as hysteria, tetanus, hydrophobia, delirium tremens, in most of which affections I have prescribed it with decided benefit. A remarkable property it possesses of reducing the frequency of the pulse in delirium tremens has been alluded to by my distinguished friend Mr. Butcher; in some cases bringing it down to fifty, or even forty in the minute; this is a statement which I have myself frequently verified. In sea-sickness it has been found very efficacious in some cases, whilst in others it has totally failed; it should be given in five or ten minim doses, with or without a little brandy; it has been also employed in the treatment of spasmodic cholera. In many of these cases pure chloroform is preferred, in others either the spiritus chloroformi or chloric ether is a favourite formulary. *Externally applied* it allays pain and local irritation, and therefore constitutes a useful addition to liniments or ointments in neuralgia, muscular rheumatism, and cutaneous diseases attended with itching, especially prurigo, chronic eczema, urticaria, and lichen.

"But it is from its effects when *inhaled in the form of vapour* that

chloroform has become so important a therapeutical agent. Towards the close of the year 1846 the discovery was made in the United States of America, that a state of partial coma with insensibility to pain could be produced by the inhalation of the vapour of sulphuric ether, and this discovery was rapidly taken advantage of for the purpose of preventing any suffering to the patient during surgical operations. It was almost immediately found, however, that either inhalation was very uncertain in its effects, producing in many persons violent excitement, spasmodic action of the muscles, delirium, and in some instances death even following its employment, and even when it produced its effects after the most to be desired fashion, that its taste and odour hung about the patient for days after its employment. The attention of the members of the profession in all parts of the world was therefore at once actively engaged, with the view of discovering a safe and effectual substitute for it; the honour of this, one of the most important discoveries of modern times, fell to the lot of Sir James Y. Simpson, of Edinburgh, who, in November, 1847, ascertained that chloroform possessed the desired properties.

"The vapour of chloroform, when inhaled in quantity not exceeding that evolved by half a drachm, produces a feeling of fulness in the head, dizziness, and partial loss of consciousness, with usually pleasurable sensations: the effects varying according to individual temperament, but in all they more or less resemble semi-intoxication. If the quantity inhaled be augmented, total insensibility is quickly produced, usually in from thirty seconds to two minutes, in some instances the patient going into a state of profound insensibility in the most imperceptible manner possible without the slightest trouble to the person administering it, but in the majority of cases, and especially so in robust individuals, a period of excitement precedes the anæsthetic stage, laughing, talking, struggling, &c. The patient frequently talking in foreign languages, but never in any instance to my knowledge, not even in that of the most abandoned characters, making use of obscene language. The stage of insensibility is marked by slight stertorous breathing, muscular relaxation and fixing of the eyes, which are generally turned upwards with their pupils slightly contracted, unless, indeed, the effects of the chloroform have been pushed to an extreme, unnecessary, and undesirable extent, when the pupils become dilated. If the inhalation be now stopped, perfect consciousness will be restored usually in from five to six minutes, the individual recovering without any remembrance of what had taken place, and in the majority of instances without any unpleasant after consequences; occasionally, however, the patient will complain of slight giddiness and headache, which, however, rapidly disappear. The circulation is somewhat affected during the induction of anæsthesia, at the commencement of the inhalation being increased both in volume and frequency; but as the effects of the chloroform are more fully developed, the strength of the

pulse becomes generally diminished, while its frequency is still slightly increased, though not nearly so much so as in the earlier stages of the inhalation. The anæsthetic condition may be kept up for hours with impunity, as is often done in parturient females, by a cautious continued use of the inhalation.

"The therapeutical applications of the inhalation of chloroform are sufficiently manifest, its effects being so fully explained above; but the purposes for which it is specially used require to be shortly noticed, namely, the prevention of pain during surgical operations, the alleviation of pain during paroxysmal attacks of it in disease, and in child-birth; so that we can advantageously consider its employment under these three heads—its *surgical*, its *medical*, and its *obstetrical* value. At first much opposition was given to the employment of anæsthetic agents for the induction of insensibility during surgical operations, and the occurrence of an occasional fatal case, even where chloroform had been administered with all due precautions, still affords its opponents an argument against its use; but the magnitude of the boon conferred is so great, and the proportionate risk of ill effect so small, that it is now used almost universally by surgeons, and by some even in the most trivial operations, in which, however, I conceive its employment is as unjustifiable as it is uncalled for—the great majority of fatal cases occurring during its administration having taken place where chloroform has been exhibited to relieve the pain of some trifling operation, such as tooth extraction, &c. Why this should be so, is, I believe, explained by the fact of sufficient attention not having been paid to keeping the patient in the recumbent posture during its administration. There is one class of operations, the reduction of dislocations, in which it not only prevents pain, but by its relaxing effect on the muscular system removes all difficulty in the reduction, so that the complicated apparatus of compound pulleys, &c., is very rarely indeed required. In the reduction of strangulated hernia, it is an important item in the *taxis*, and when employed as such, in case of failure should only be a preliminary step to operation, the surgeon at once proceeding to relieve the stricture, without allowing the patient to recover from the state of anæsthesia, inasmuch as it is manifestly uncalled for to subject the patient to a second risk from the administration of chloroform; and in the introduction of a catheter in spasmodic stricture, its relaxing effects are also especially advantageous. In operations about the mouth and nose only does the production of anæsthesia seem to be contra-indicated, and this depends on the danger that might result from the flow of blood into the air-passages during the insensible state of the patient. But with proper precautions even under these circumstances, its use is attended with the greatest advantage. I have on more than one occasion administered it to patients undergoing operations for the removal of the upper jaw, keeping them for forty or fifty minutes in a state of complete unconsciousness, without any unpleasant consequence ensuing.

"In the practice of medicine chloroform has been employed with the view of producing anæsthesia, with varying, but for the time most favourable, results in the treatment of tetanus, delirium tremens, hysteria, chorea, neuralgia, and such like painful affections, and in relieving the exacerbations of pain in cancer and other malignant diseases; it has also been had recourse to in hydrophobia, but although the spasms and suffering are thereby temporarily alleviated, no decided impression is made on the fatal progress of the disease. We occasionally meet with cases of intense pain, where the suffering is so great as to prevent anodyne medicines developing their beneficial effects. In such cases the exhibition of the vapour of chloroform arrests the pain, and admits of time for anodyne to act. In such cases I have frequently succeeded in procuring for the sufferer several hours of comfortable sleep, by first giving a full dose of opium, and then controlling the pain by the anæsthetic action of chloroform. In relieving the agony attendant upon the passage of gall-stones, as also of renal calculi, I know of no medicine so valuable. In a case of hay asthma I have seen the access of the attack kept off by constantly smelling chloroform, which the patient carried about with him for the purpose; and in hooping-cough I have very frequently indeed seen good results follow its inhalation; a few drops are to be placed in the palm of the nurse's hand, and the little patient allowed to breathe it; in a few moments its beneficial action will be evidenced. In laryngismus stridulus a similar proceeding is attended with the happiest results.

"It is, however, to the use of chloroform in midwifery that most opposition has been given, and since it was first employed by Sir James Simpson, a fierce controversy has raged between obstetrical practitioners on the subject, the opponents founding their objections upon two grounds—medical and scriptural—stating with reference to the first, that, in addition to its other possible ill effects, it predisposes to hemorrhage after delivery; and, with reference to the second, that pain is laid down by the law as woman's penalty for original sin. Scripture authority, however, has been brought to bear on both sides of this latter question. But as I am not myself a practitioner in midwifery, and consequently cannot speak from personal experience, nor yet a theologian, I wish merely to deal with facts. In Edinburgh chloroform is employed—to speak in general terms—in *every case* of labour, natural or preternatural, and with safety to both mother and child, its anæsthetic effects being pressed during the pains, and withheld during the intervals; while the opinion of the majority of accoucheurs in this and most other large cities, as far as I can judge from what has been written on the subject, is well expressed in the following extract from the third edition of Dr. Churchill's *Midwifery*:—As to its exhibition in *natural labour*, as I do not believe that in the large majority of cases convalescence is at all impeded by the suffering, I cannot see the necessity, or even the propriety, of urging the employment

of anæsthesia in every case; and I do feel that even greater caution ought to be used than in operative midwifery. We may be justified in running some risk when an important point is to be gained, such as perfect quietness during an operation, which we should not be justified in incurring merely to relieve pain. The most recent authority on the subject, Professor Sinclair, in an able paper in the seventy-fifth number of the *Dublin Quarterly Journal*, thus records his opinion:—'In fact there exist three opinions on the subject, namely:—Firstly, that it should be given in all labour cases; secondly, that it should be administered only in certain selected cases; and thirdly, that it should never be given at all. Prejudice, may, of course, influence each section of opinion; to me the middle course appears to be the one most consonant with reason. To assert that because the indiscriminate administration of chloroform in obstetric medicine has sometimes proved dangerous or even fatal, it should be therefore excluded from obstetric practice altogether, is simply to argue against its use from its abuse.' 'Now, although amongst the facts here recorded not one fatal accident from chloroform can be found; though it cannot be said that mortality was increased in any way by its means; or that disease, on account of its exhibition, was rendered more rife, or convalescence prolonged; though evidence sufficient cannot be obtained, from the perusal of these observations, to cause its utter condemnation and expulsion from obstetric practice; still, in my opinion, sufficient *can* be gleaned to enable us to come to the conclusion, that the indiscriminate exhibition of chloroform vapour in labour cases should be abandoned, and that it should never be given in labour purely natural, or nearly so. It is true that out of all the cases Dr. Johnston and I have recorded, derived from our hospital experience, but on two or three occasions did symptoms sufficiently alarming occur to cause us to desist in its administration; and it is equally true that from out of my own private practice I can adduce but two cases strongly contra-indicating it; still these two, taken alone, are quite sufficient, in my opinion, to sever chloroform from its much too intimate connexion with natural labour.' It is right, however, to add, that in no instance has a fatal result followed the inhalation of chloroform in midwifery practice. With reference to what, after all, is the most important portion of the question, its tendency to favour flooding after delivery, Dr. Beatty, with many other authorities, entertains no doubt but that it is open to this charge; he has shown us, however, how we can divest it of this serious drawback (by preceding its use with that of ergot of rye) in his valuable *Contributions to Medicine and Midwifery*, p. 190.

The circumstances generally taken into consideration as *modifying the action of chloroform* are age, strength, and disease. Of these it may be stated that the younger and the older the patient is, the more likely is he to prove amenable to the influence of chloroform. In this instance, as

in many others, extremes meet; at these ages we rarely read of fatal cases, and this statement also holds good as to strength. The debilitated and weak, either in consequence of age or disease, bear its administration better than the robust, whilst so far as disease is concerned, my experience (now a rather extended one) agrees with that of Snow—that no matter whether it be disease of brain, lung, heart, or of the large blood-vessels, if its administration be required by the emergency of an operation, a fatal result is less likely to follow its administration than from the shock under such circumstances of a capital operation when the patient is not under its influence. In cases of suspected weak heart, its anæsthetic employment should always be preceded by the administration of some alcoholic stimulus;—a most valuable practical hint for which the profession is indebted to my friend, Mr. Fleming, of this city. In addition to these, however, other circumstances require consideration; one of these which modifies the action of chloroform has not hitherto been alluded to, for the simple reason that the contingency requiring its consideration does not frequently arise. I allude to the influence that habit might exercise over its power of inducing anæsthesia. In one case which was under my care this question was to a certain extent solved; it was that of a lady dying in excruciating agony of cancer of the uterus; the only medicine that could relieve the intensity of her sufferings was the inhalation of chloroform, of which at last her constitution got such a tolerance that she consumed daily from 12 to 16 ounces. That some was wasted may be admitted, but the amount of waste was reduced to a minimum by the employment of Skinner's pipette and mask. The other circumstance modifiying the anæsthetic action of chloroform, the influence exercised by race, deserves special consideration. It had been long observed that race to a certain extent modifies the action of medicines, but up to the present, so far as I am aware, no observations have been placed on record in connexion with chloroform upon this subject; therefore I feel peculiar pleasure in submitting for my readers' consideration a *resumé* of the careful observations of so acute an observer as Dr. Lyons, of this city, upon this point. As the result of a wide experience in the Crimea, where it will be remembered he was pathologist in chief to the British forces, Dr. Lyons specially remarks on the very great variety which appears to be observable in regard to the readiness or the contrary with which the system lends itself to the influence of chloroform, partially due to individual idiosyncracy, and on a larger scale probably to national peculiarity of nervous temperament. Thus some persons will be found to sink quickly and silently into the anæsthetic state, while others first exhibit a condition of high excitement, with wild and occasionally furious gesticulations and efforts to free themselves from all control—shouts, curses, and every species of angry vociferation being exhibited in one set of cases, while in another, rambling, silly, and incoherent attempts at narrative, description

or conversation, intermixed with peals of laughter, mark the stage of excitement. A return to the use of a native though long unused patois or language is a characteristic often noticeable. Taken in order of susceptibility the Turks and the Russian prisoners, gentle and docile to a degree little in keeping with the popular notion of the wild soldier of the fierce Tartar horde, quickly and readily inhaled the soothing vapour, and with hardly an exception fell silently, in the space of a few minutes, into a state of complete and profound anæsthesia, which lasted till all the stages of the most formidable capital operations of the field were fully completed. In the Russian, then, it may be stated the exhibition of chloroform was immediate in its effects, entirely and perfectly successful in annihilating all sense of pain, and operations, however difficult, were facilitated and expedited. The phlegmatic, blue-eyed, fair-haired Englishman, of Saxon type, next deserves mention. His susceptibility to chloroform was not so quickly exhibited nor so quietly accomplished as in the case of the Russ; but yet in general the vapour was borne without struggles against its inhalation, anæsthetic sleep was induced after a moderate interval, and was usually complete to the end of the operation. The Sardinian may be stated to occupy an intermediate place between the ready susceptibility of the Turk, the Russ, and the Saxon Englishman, *pur sang*, on the one hand, and the more vivacious, mercurial, and nervously excitable temperaments of the French and Irish specimens of the great Celtic brotherhood. Resistance to inhalation, with more or less occasional excitement in gesture and language, marked the exhibition of chloroform in the North Italian, but was never so extreme or attended with such violent demonstrations and such infinite trouble to the operator as in the French or Irish. In the French, of whom the agile Zouave might be taken as a type, the inhalation of the vapour was very often resisted after the first effort or two, a stage of struggles with wild excitement, rambling, incoherent, and boisterous talk, often in patois, with shouts, laughter, or curses, very frequently ensued, and it was not till after a protracted interval that anæsthesia was finally induced, and then was hardly ever so profound or so prolonged as in the Turk, the Russ, or the Englishman. It is to the Hibernian Celt, however, that we must refer for the most marked exhibition of all the phenomena of chloroformic excitement, and its wildest demonstrations. Inhalation was stoutly resisted, and when partially effected, soon gave evidence of its exciting and intoxicating effects by furious struggles, curses 'both loud and deep,' anger in one case, risible excitement in another, and finally a voluble outpouring of the native Irish, marked by the rich brogue of the southern, or the harder clang of the northern Irishman. The amount of time consumed in such cases before the patient was reduced to a condition of anæsthetic quiet was in many instances very great, and became the source of infinite

trouble, but what was of far more consequence, entailed irremediable suffering and loss of life on those whose wounds could not be attended to in time. The Scotch Celt exhibited in but a partial degree the excitement so marked in his Irish brother. The duration of this stage of chloroform intoxication varies much, and is sometimes prolonged to a period that, as Dr. Lyons observes, renders the employment of the drug a serious drawback to the comfort, if not to the safety to life of other sufferers, when as in time of war many have to be operated on, and the number of surgeons and assistants is limited. It is questionable whether under such circumstances chloroform should be employed at all. Dr. Lyons states that after the memorable engagements which terminated in the fall of Sebastopol, the time necessarily consumed in the administration of chloroform led to the loss of many lives, in the cases of individuals whom it was found impossible to operate upon within the period in which primary amputation is admissible.

" If poisonous symptoms ensue on the *internal* use of chloroform, they should be at once met with the administration of emetics and stimulants; this, however, is a rare occurrence. They generally supervene on its exhibition as an *anæsthetic* agent, and may be suspected on the superventiion during its administration of heavy, stertorous, interrupted breathing, dusky livid hue of countenance, and intermitting faltering pulse. On the moment the chloroform should be removed, a free current of air admitted, the tongue drawn forward, and, if necessary. artificial respiration and the electro-magnetic current should be had recourse to. The following remarks of Dr. Snow are so pertinent that I introduce them here :—Such measures as dashing cold water on the patient, and applying ammonia to the nostrils, can hardly be expected to have any effect on a patient who is suffering from an overdose of chloroform ; for they would have no effect whatever on one who has inhaled it in the usual manner, and is merely ready for a surgical operation, but in no danger. I have applied the strongest ammonia to the nostrils of animals that were narcotized by chloroform to the third or fourth degree, and it did not affect the breathing in the least. They recovered just as if nothing had been done. It is difficult to suppose a case in which the breathing should be arrested by the effects of chloroform whilst the skin remained sensible, yet it is only in such a case that the dashing of cold water on the patient could be of use. There is, however, no harm in the application of this and such like means, provided they do not usurp the time which ought to be occupied in artificial respiration; for this measure should be resorted to the moment the natural breathing has entirely ceased.

" *Dose and Mode of Administration.—Internally* in the fluid form, min. v. to min. xxx. suspended in water by means of mucilage of gum acacia, or of gum tragacanth, or of Irish moss as proposed by the late Professor Osborne, but which does not answer as well as either of the gums. In

consequence of the volatility of chloroform, it should be always prescribed in droughts. *Anæsthesia* is usually produced by the inhalation of the vapour emanating from f℥j. to f℥ij. It is effectually and safely administered in the manner first proposed by Sir J. Y. Simpson, namely, by pouring the chloroform into the hollow of a handkerchief folded in the form of an inverted cone; at first f℥ss. only should be used, and if the desired effect be not produced in about two minutes, the same quantity may be renewed. Various forms of *inhalers* have been proposed for the administration of the vapour of chloroform, but I must confess that I prefer the simple handkerchief, or the form of inhaler recommended by Dr. Skinner, of Liverpool, which, after all, is but a convenient modification of the handkerchief, and which allows the thorough admixture of atmospheric air with the vapour; his plan of dropping the chloroform from a bottle by means of a pipette is, in my opinion, a vast improvement; this is the way in which I have invariably administered chloroform latterly for anæsthetic purposes. The chief points to be attended to are—1st. That the patient should be lying on his back with the head slightly raised. I have designedly put this point first, as I consider the recumbent position a *sine qua non* for the safe exhibition of chloroform. Should the necessities of the operation require the patient to be sitting up, he should be first anæsthetized in the recumbent position, and then be raised up to any required extent. 2nd. That he should be permitted at first to breathe atmospheric air freely mixed with the chloroform, which is effected by at first only dropping a few minims through the pipette on the Skinners' inhaler, which, as the anæsthesia is developed, can be increased in number, and by not bringing the chloroform too close to the mouth and nose at once. At this period the party administering the chloroform should bear in mind the effect temperature has in modifying the amount of vapour of chloroform that the atmospheric air will take up. At 60° F. an atmosphere *half* saturated with chloroform vapour will contain 6 per cent. of chloroform vapour, whilst at 80° F. an atmosphere half saturated with chloroform vapour, will contain 13 per cent., or more than double the amount of chloroform vapour (see p. 495). 3rd. That the vapour should be altogether withdrawn as soon as insensibility is produced, which is usually evidenced by the occurrence of slight stertorous breathing, for this condition can be kept up for any length of time that may be requisite, by the occasional reapplication of fresh chloroform on the handkerchief or inhaler. 4th. That the patient's stomach should be empty when the inhalation is commenced, as otherwise vomiting is apt to be produced; we can secure this point by directing the last meal to be given some three or four hours previous to the exhibition of the chloroform; should unfortunately emesis occur during its exhibition, the patient should be turned on his side, and his mouth directed downwards so as to facilitate the ejection of the contents of

the stomach, otherwise a portion of the vomited matter might get into the windpipe and produce speedy death by asphyxia. 5th. That in public operations, as in hospitals, the patient should, if possible, be anæsthetized before being introduced into the theatre; the excitement consequent on facing a crowd frequently opposing the successful administration of chloroform. In all cases the party administering the chloroform should devote his entire and undivided attention to his patient. From inattention to this point, more than once have I seen a patient's life jeopardized. Every article of clothing (tight strings, &c.) that would embarrass the patient's breathing should be removed; on no account should any pressure on the chest or abdomen be permitted; and perfect silence should be enforced during its exhibition; this, however, need not prevent us allowing the patient himself to talk, inasmuch as encouraging him to talk facilitates his coming more rapidly under the influence of the chloroform, a fact first pointed out to me by my colleague, Dr. Wharton. There is but one other remark which I have to make to those inexperienced in the use of chloroform, namely, that during the process of inhalation, just before insensibility is produced, there is usually a struggle on the part of the patient; *this must be resisted, and the chloroform kept just at the time closely applied to the mouth and nostrils.* In surgical operations, as a ready means of ascertaining when the patient is sufficiently insensible to permit an operation to be commenced, Dr. Snow employs as a test, touching the ciliary edge of the eyelids, and when this does not occasion winkling, then the insensibility is sufficient. In the United States of America, a mixture of three parts of ether and one part of chloroform, is ordinarily employed to produce anæsthesia, which it is said to do effectually and without risk; to this mixture, however, Snow objects, that in consequence of the greater volatility of the ether over the chloroform, we have a most objectionable compound—the ether firstproducing its effects, and subsequently, at the most dangerous period of the anæsthesia, the chloroform coming into play. M. Bourguignon has recently proposed to substitute the vapour of ether for that of chloroform, to keep up the state of insensibility as soon as anæsthesia is produced by the latter. For *external* use the chloroform liniment of the Pharmacopœia answers admirably, one part of it mixed with three parts of the belladonna liniment, forms an admirable embrocation for lumbago and other muscular pains; or from min. xx. ro f℥j. may be added to ℥j of some direct mild ointment. Dr. Hardy, of this city, has proposed the direct application of the vapour of chloroform in painful affections, especially those of the uterine organs, from its use in which he has seen much benefit result; for this purpose he has invented an ingenious but simple instrument. (See *Dublin Quarterly Medical Journal*, vol. xvi., page 306.")

Dr. Macnamara is himself no mean authority on the subject of

Anesthetics, and, therefore, we regret that he did not extend his observations relative to these agents to the various chlorides of carbon which have lately been proposed as substitutes for chloroform. In the next edition we hope to see an entire chapter devoted to the subject of Anesthetics, for they must surely be regarded as something more than "sedatives or contra stimulants."

We have, so far, only complimented the author for the creditable manner in which his part of this work has been executed; but we cannot conclude without a word of praise in favour of the printer and publishers. A remarkably clear, well-faced type, very white, smooth, and highly-finished paper, and extremely neat binding, go far to make this large volume as attractive to the eye, as Dr. Macnamara's labours have rendered it interesting to the intelligence of the reader. We are quite satisfied that the work will have a circulation even more extensive than that attained by any former edition; and it gives us sincere pleasure to learn that it has been made one of the class books officially recommended to the medical students of the University of Dublin.

On State Medicine in Great Britain and Ireland. By HENRY W. RUMSEY, M.D., F.R.C.S., &c., Lond. 1867. Pp. 58.

To render truly valuable the knowledge obtainable from medical registration, both of death and sickness:—to bring the full benefit of medical knowledge to bear upon coroner's inquests and to elucidate, in a recognized manner, all investigations of a medico-legal nature—and to erect into a special machinery, for the prevention of disease, the various resources within the present and daily increasing knowledge of the profession — in short, to hold out inducements to competent medical men to fulfil the triple duties of medical investigation, and registration of sickness and death, of medical jurist, and that no less important branch of our profession, hygiene, is the object which Dr. Rumsey has ably undertaken to show the importance of; and we congratulate him on his work, and the valuable assistance he has obtained from the sanction of the British Medical Association.

With the name of Dr. Rumsey we would associate those of Dr. Farr, Mr. Ransome, Dr. Symonds, and others, who have laboured to bring medicine into that just relation to the state, that it merits, and that the best interests of the community require.

We will not deny that Ireland derives much benefit from the machinery of her medical charities, but much remains undone. The dispensary medical officers offer a system of organization available for the prevention of disease, unsurpassed, if not unparalleled, in Europe; yet not only does the country not avail itself of their services for this most important service, and hold out the inducement of suitable recognition and pay, but it permits those members of the public service who do offer suggestions for the improvement of the sanitary, and, therefore, of the moral and social condition of the humbler classes, to be subjected to the hostility of prejudiced persons, who, in some instances, have actually succeeded in depriving meritorious officers of their posts, inflicting a serious loss upon the community, and permitting incalculable evil to the sick poor, as well as preventing other officers from exerting themselves in behalf of those whom the state should care for, not only on principles of humantiy and morality, but of policy.

Since the foregoing was put in type a copy of the following extract was sent to each Dispensary Medical Officer in Ireland; from it, as it will be seen, the Legislature places highly responsible additional duty upon him without "fee or reward." This important service, attended with no small personal risk, as well as with frequent heavy calls upon the Dispensary Medical Officer's time, will be all the more felt by him, inasmuch as it is to private practice he must look for emolument, rather than to the very small remuneration he receives at present for his already most onerous and trying duty. We trust, however, that the authorities will reconsider the case of their Dispensary Medical Officers, and grant them a scale of pay commensurate with the attainments they are required to possess, and the amount of duty, responsibility, and danger encountered by them:—

"*Copy of the* 10*th Section of the Act* 30 *&* 31 *Vic. c.* 118.

"From and after the First day of *January* One thousand eight hundred and sixty-eight whenever any Person shall be brought before any Two Justices of any County, County of a City, County of a Town, City or Town, and it shall be proved to their Satisfaction that such Person was discovered and apprehended under Circumstances denoting a Derangement of Mind, and a Purpose of committing some Crime for which, if committed, such Person would be liable to be indicted, the said Justices shall call to their Assistance the Medical Officer, or if there be more than One, the nearest available Medical Officer of the Dispensary District in which they shall be at the Time, and if there shall not be any such Medical Officer available, then the nearest available Medical Officer of any neighbouring Dispensary District, who shall examine such Person without Fee or Reward; and if such Medical Officer shall certify that such Person is a dangerous Lunatic or a dangerous Idiot, it shall be lawful for the said

Justices, by Warrant under their Hands and Seals, to direct that such Person shall be taken to the Lunatic Asylum established either wholly or in part for the County, County of a City, or County of a Town in which he shall have been apprehended ; and every such Person shall remain under Confinement in such Asylum, and be there maintained, in like Manner and subject to the same Conditions as if such Person had been removed from any Gaol to such Asylum by virtue of the Warrant of the Lord Lieutenant under the Provisions of the recited Act of the First Year of Her present Majesty, Chapter Twenty-seven : Provided always, that nothing herein contained shall be construed to restrain or prevent any Relation or Friend from taking such Person under his own Care and Protection if he shall enter into sufficient Recognizance for his or her peaceable Behaviour or safe Custody before Two Justices of the Peace, or the Chairman of the Court of Quarter Sessions of the County in which such Person shall be confined, or One of the Judges of Her Majesty's Superior Courts at *Dublin*."

We will conclude this brief notice by referring our readers to the work of Dr. Rumsey, and now append the resolutions adopted by the British Medical Association in August last.

Resolutions adopted by the Association, August 7, 1867.

I. " That the Association desires to express its decided approval of the plan proposed by Dr. Farr; namely, the appointment of a registration medical officer in every registration district or group of districts, with medico-legal and sanitary functions, and pledges itself to support that measure, as the initiative step to a national organization for purposes of of state medicine.

II. " That the Committee of Council be instructed to direct their early or special attention to the amendment of the sanitary laws ; to invite the co-operation, for this end, of the Council of the National Association for the Promotion of Social Science ; and to urge the Branches of this Association to promote the same important object by local efforts, by representation to individual Members of Parliament, and, if need be, by deputations, to Her Majesty's Government.

III. " That a Committee be appointed[a] . . . to collect information, in such manner as they may think best, on the subjects brought before the Association by Dr. Rumsey; and that the Committee of Council be empowered to make such pecuniary grant as the funds of the Association will permit towards the expenses of this inquiry."

[a] COMMITTEE.—Dr. Acland, F.R.S., *Chairman* ; Dr. Burke, Dr. Falconer, Dr. Gairdner, Mr. Earnest Hart, Dr. Lankester, F.R.S., Dr. Mapother, Dr. J. E. Morgan, Dr. G. H. Philipson, Mr. Ransome, M.B., Dr. Tindal Robertson, Dr. Rumsey, Dr. Symonds, F.R.S., Ed., Dr. A. T. H. Waters; Dr. A. P. Stewart, *Hon. Secretary.*

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE KING AND QUEEN'S COLLEGE OF PHYSICIANS.

Multiple Abscess of the Liver—Evacuation by Puncture at the Epigastrium—Subsequent Irruption into the Pleura—Paracentesis, with Introduction of Drainage-Tube—Death. By THOMAS HAYDEN, F.K.Q.C.P.I., Physician to the Mater Misericordiæ Hospital.

(Read before the Medical Society, Dec. 18, 1867.)

FRANCIS WÖBER, of German extraction, aged forty-four, was admitted into the Mater Misericordiæ Hospital June 26, 1867; had served twenty-one years as a soldier in the Presidency of Bengal, whence he returned to Ireland eighteen months ago.

Some months after arriving in India he had brain-fever, but, with this exception, had suffered no serious illness whilst there; had been liable to bilious attacks whilst in India, and since his return to Europe has been frequently "bilious."

About three weeks prior to the date of admittance he experienced a general and indescribable uneasiness in the region of the stomach, and in the back and shoulders; and a week subsequently he observed a prominence in the epigastrium; since then he has had frequent chills, but no regular rigor.

When admitted he was slightly icteric, thin and weak; the pulse was slow and feeble; bowels confined; he perspired at night, and lay only on his back; a prominence existed in the epigastrium, which was dull on percussion, as was likewise the surface to an extent of two inches below; tender, soft, and yielding to firm pressure, whilst above and to the right of this prominence the feeling yielded was that of a hard and solid body; the inferior right costal cartilages were pushed out, and hepatic dulness was extended upwards about two inches on right side, and in middle line.

From day to day the epigastric prominence increased in size, and yielded stronger evidence of the presence of liquid. On the 3rd July satisfactory evidence of adhesion to the abdominal wall having been obtained, and the liquid contents of the tumour having approached quite near the surface, a puncture was made with a sharp straight bistoury, and about a quart of grumous pus, mixed with some dark blood, was discharged.

As a means of evacuating the abscess, and subsequently of obliterating its cavity, a binder was previously applied, as in the operation for ascites.

For some days prior to the puncturing of the abscess the patient had been perspiring profusely; was weak, and required a sedative to procure even a short sleep; he had been taking a pill three times daily, consisting of extract of taraxacum, dried soda, and blue pill, in equal proportions. Nitro-hydrochloric acid was subsequently given in infusion of chiretta, eight ounces of wine and four of brandy were taken daily, and strong beef-tea with bread crumb for diet. The daily discharge from the opening made in the epigastrium was very considerable, of a dark flaky character, and gushed from the wound when the patient coughed. The right hypochondrium, which, previously to the performance of the operation, was remarkably prominent, subsided to its normal level; the tongue became clean, and the appetite and sleep improved. Such was the patient's condition on the 13th July. At four o'clock on the morning of the 14th he was suddenly attacked with acute pain in the lower portion of the right side, which deprived him of sleep. When visited on that morning he was suffering from sharp stabbing pain in the right side, which he referred to the infra-mammary and lower lateral regions on making a full respiration. This side was dull to within an inch of the nipple, and to the same extent vocal fremitus was absent; a loud friction sound existed laterally below the level of dulness; pulse 102, and sharp; *abscess discharging freely through wound.*

The diagnosis was at once made, of irruption into the pleural cavity, of an abscess distinct from that which had been opened in the epigastrium. The latter portion of the diagnosis was based mainly upon the continuance, in undiminished quantity, of the discharge from the abdominal puncture; to have one grain of calomel and three of James' powder every alternate hour; the side to be cupped in the seat of pain to three ounces, and subsequently covered with a warm poultice.

July 26.—Last night Dr. Curran, who was doing temporary duty for the Resident Clinical Assistant, Mr. Finnegan, was hastily summoned to Wëber about two o'clock, and found him struggling for breath, bathed in perspiration, and all but pulseless; the extremities were cold and clammy. Dr. Curran administered a stimulant draught, and the patient rallied. On making my morning visit I found him pale and haggard-looking;

pulse 132, and weak; respiration 36, and laboured; surface cold and moist, and the sheets, which only a few hours previously had beenput on to replace others saturated with perspiration, were also quite wet from the same cause; urine free from bile; no jaundice; lower portion of right side was projected, and nearly motionless; intercostal spaces obliterated; dulness extended upwards to within two inches of the clavicle, and to the same extent vocal vibration was abolished, and respiratory sounds were absent, save by transmission; beneath the clavicle resonance was somewhat metallic; and here there was thoracic movement, and respiration was bronchial; dulness extending horizontally only to right margin of sternum; heart aching tumultuously. A large aperture, readily admitting the finger, had been formed by a process of sloughing in the site of the wound in the epigastrium, from which a flaky, ichorous matter was freely discharged, and when the patient coughed, burst forth with considerable force. Since last report cupping has been repeated with the view of alleviating pain, and blisters have been applied to the side; brandy has been given to amount of 10 to 12 ounces daily; there has been irritability of stomach, to allay which hydrocyanic acid was given with good effect.

Being apprehensive that the patient might die of asphyxia in the course of the ensuing night, if not relieved by paracentesis, I decided upon the operation, and had the advice and assistance of my colleague, Dr. Cruise, who agreed with me as to the urgency of the case, and the necessity for operative interference. The operation was performed at four p.m., as previously arranged, and in the following manner:—An incision was made through the integument and muscles in the seventh intercostal space, a short distance in front of the angles of the ribs, and through this a capillary trochar and canula were introduced. On withdrawing the trochar some dark brown serum escaped; and after a few seconds this became mixed with flakes of pus. It was at once decided to introduce Chassaignac's drainage-tube, which was accordingly done, with some difficulty, by means of the usual instrument, the counter opening being made also in the seventh intercostal space, about two inches in front of the primary wound.

A considerable discharge of pus took place through the tube; the patient felt relieved by the operation, and passed a comparatively good night; he took stimulants and light nourishment freely; perspiration was checked; respiration was less embarrassed; and the superior limit of percussion-dulness had descended an inch and a half on the following day; *but with this exception the percussion and stethoscopic phenomena remained unaltered;* the discharge from the drainage-tube was limited, and entirely purulent.

The relief obtained from the operation was only temporary, and on the second day succeeding the patient began to sink, and died of asthenia at

2.20 p.m., on the 30th July, *i. e.*, four days after the date of performance of the operation. The body was examined on the following morning. A large quantity of dark brown serum was found in the right pleural cavity, mixed inferiorly with thick, flaky pus; the mediastinum was pushed to the left, and the liver, covered by the stretched and attenuated diaphragm, projected into the thorax nearly to the level of the second rib; the lung was forced by the pressure of the liver, and of the liquid effusion, into the superior and posterior portion of the pleural cavity, and its apex was firmly attached to the cone of the pleura. There was likewise considerable serous effusion into the pericardium, and to a less amount into the cavity of the peritoneum. The liver was greatly enlarged, weighing, when removed from the body, 7 lbs. 2 oz.; it was of a dark chocolate colour in a few places, but elsewhere of the natural tint; the right lobe was elongated vertically, and on its anterior-superior surface was an abscess capable of containing an orange; this abscess was bounded superiorly by the diaphragm, which was firmly attached to it, and attenuated to the consistence of a thin film composed only of its serous investments; through this abscess, including its serous coverings, the drainage-tube had been passed; the cavity of the abscess contained a small quantity of thick, flocculent pus; its walls were rugged, and no communication with it existed, save that through the drainage-tube.

Another abscess of much greater magnitude was found on the posterior thick edge of the liver, occupying the notch for the reception of the vertebral column, and projecting into the pleura; it was invested by the diaphragm much reduced in thickness; was full of curdy pus; fluctuating on the surface, and manifestly on the point of bursting into the pleura; it had no communication with the former.

On the lower portion of the anterior surface of the right lobe, near the thin edge of the organ, and occupying in part the notch for the round ligament, was a third abscess, not extending deeply into the liver, but communicating with the wound made in the epigastrium, now much enlarged by the process of sloughing; this abscess was the smallest of the three, and had no communication with either of the former; it must have been of much larger size when punctured, as it then yielded at least a quart ofpus.

The interest which this case possesses has reference chiefly to the diagnosis. Previously to the performance of paracentesis it was impossible to determine accurately the height to which the liver ascended in the chest; this was due to the attachment of the lung to the upper portion of the anterior wall of the thorax, by which it was prevented from receding before the encroaching liver, and yielded fallacious evidence in regard to the superior limit of the latter. The occurrence of pleuritis with effusion, consequent, in all probability, upon the escape into the pleura of a portion of the contents of one of the hepatic abscesses

on the morning of July 14th, increased the height of percussion-dulness by still further compressing the lung; and when the liquid was removed by tapping, the comparatively small quantity discharged, and the descent of the level of percussion-dulness by an inch and a half only, and without collapse of the side, showed plainly that the liver occupied the greater portion of the pleural cavity.

The question as to the necessity for paracentesis having been decided affirmatively, it was impossible, by physical examination, to determine the proximity of the liver; or, in other words, to distinguish the hepatic dulness from that due to the liquid effusion. Nor is it at all certain that, however much the penetration of the abscess by the drainage-tube may have increased the apparent gravity of the operation, its danger was thereby in any measure increased; for the tube, by affording a ready channel of exit for the matter, prevented its escape into the pleura; and the transfixion of the diaphragm, reduced as it was to its serous coverings, could scarcely be regarded as a complication in a case otherwise so serious.

Owing to the penetration of the abscess by the instrument used for the introduction of the drainage-tube, it was impossible to determine with certainty from *post mortem* evidence, whether the pleuritis was caused by the escape of matter into the pleura, or by the irritation due to the pressure of the abscess, separated as it was from the pleura only by a thin film of membrane; for, assuming the abscess to have given way, the small opening so made might have been occupied by the instrument either in passing into or out of it; and the small quantity of matter found in the pleura might have escaped into it from the tube. Nor was it possible to decide whether the trochar used in puncturing the chest had entered the abscess, because the opening so made might have been likewise occupied by the larger instrument subsequently introduced. I feel warranted, however, in concluding that the matter had found entrance into the pleura previously to the operation, and that the trochar did not enter the abscess, by a consideration of the following circumstances, viz., the *sudden* occurrence of acute pain in the side on the morning of the 14th July, followed, within a few hours, by the signs of effusion into the pleura; and, secondly, the escape through the canula, first, of clear serum, and then of pus, an order of events which should have been inverted, had the trochar, in the first instance, entered the abscess.

Rouis, as quoted by Frerichs, states that of 50 cases which occurred in his practice in Algeria, in which the abscess had passed beyond the boundaries of the liver, 11 had evacuated themselves into the right pleura; and Moorehead observed 14 out of 140, or 10 per cent., to open into the lung or sac of the pleura. Frerichs remarks:—"Most cases where the abscess bursts into the pleural cavity, terminate fatally," and that, with few exceptions, multiple abscesses are fatal. Out of a total of

84 cases Moorehead found 49, or somewhat more than one-half, to be multiple.

The abscess on the anterior surface of the liver was firmly attached to the abdominal wall, and had approached quite near the surface before it was opened. Owing to the strong evidence of adhesion that existed, consisting in redness, pointing, and yielding on the cutaneous surface, and immobility of the tumour during forced respiration, no fear was entertained of escape of matter into the peritoneum, consequent on puncture. Adhesion of abscesses so situate is not of common occurrence. Drs. Graves and Stokes remark:—"From our own experience we are disposed to consider adhesions between the parietal and hepatic peritoneum, in cases of abscess in the convexity of the liver, as of rare occurrence; a circumstance which presents a remarkable contrast with their frequency in the thoracic cavity."

In regard to the thoracic complications, and the difficulties in diagnosis to which they occasionally give rise, Dr. Moorehead remarks:—"It is important to know that there may be empyema existing with hepatic abscess, not caused by communication, but merely by extension of inflammatory action through the diaphragm, in individuals prone to the suppurative process. It appears, then, that empyema from communication, or independent of it, is not an unfrequent complication, and it sometimes renders the diagnosis of hepatic abscess obscure; the signs of the empyema may be attributed to the encroachment of the liver on the chest, or, if rightly interpreted, they may throw a doubt over the previous diagnosis of hepatic disease."

Gangrene, to a considerable extent, occurred round the puncture in the abdomen: this is a very unfavourable prognostic. Out of 16 fatal cases of puncture, in the practice of Dr. Moorehead, gangrene took place in 13.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.[a]

Dr. Gordon, President.

Bright's Disease of the Kidney with Cyst.—Dr. Little exhibited the kidneys of a man who had died in the Adelaide Hospital, where he had been a patient for the five weeks preceding his death. He was a large pale, heavy, flabby man, 65 years of age. He was a teacher of languages, and his illness commenced, a month before his admission, with rigors after exposure to cold and wet. When admitted he was weak

[a] These reports are furnished by the Secretary to the Society.

and very much depressed, but the symptoms of which he chiefly complained were the following: severe pain across his loins, itchiness of the surface of the body, which was covered with a lichenous rash; and constant nausea, which sometimes produced vomiting. During the twenty-four hours after admission he passed 42 ounces of urine, which was smoky in colour, coagulable to one-third, of 1,010 sp. gravity, and was found to contain blood casts, free blood globules, and a large quantity of uric acid. Soon after he came into hospital the pain in the back ceased, but the lichenous rash continued, as did also the irritability of the stomach. There was urgent thirst, and the tongue and back of the pharynx exhibited the red, glazed appearance occasionally observed in the later stages of Bright's disease. After a few days the uric acid ceased to appear; in other respects the character of the urine underwent no change. Suppression, however, came on gradually during the last two days. The patient was a man of considerable intelligence, and was able to give a clear history of his case. He said that for 20 years he had been subject to similar attacks. His last illness had been in Edinburgh four years ago, and four years before that he had had an attack in London. On each occasion he had pain in the back, an itching rash over the skin, and great irritability of the stomach. There was no œdema, nor any indication of the nervous centres being affected by the disease: death took place by exhaustion. The kidneys were paler and larger than healthy kidneys—they weighed 19½ ounces, and presented a considerable number of cysts. Dr. Little considered it unnecessary to enlarge upon the mode of formation of these cysts. It appeared that they could not be formed in cases of Bright's disease which were steadily progressive, but probably were developed in cases where there had been successive attacks, each affecting only a portion of the kidney. Where the greater part of the secreting structure was implicated in the morbid process, death generally took place so early that there was not time for the dilatation of the uriniferous tubes into cysts. In such cases, moreover, the blood vessels of the gland were so extensively obliterated that they no longer conveyed to the Malpighian bodies blood enough to furnish the serous fluid, by the accumulation of which within the upper ends of the tubes whose extremities were blocked up, the dilating process seemed to be initiated. Remembering this man's history, it was not unreasonable to suppose that on each occasion when he suffered from these symptoms, namely, severe pain in the back, itchiness of the skin, and gastric irritation, he had an attack of Bright's disease, limited to a portion of the kidneys. Bright's disease was now admitted to be a constitutional affection, and he saw no reason why a small portion of the kidney might not be destroyed, just as in tuberculosis a small portion of a lung was often destroyed, and temporary arrest of the disease followed.—*December* 7, 1867.

Enchondroma of the Hand.—Dr. Barton exhibited a tumour which he had removed with part of the hand of a woman aged forty years. She was admitted into the Adelaide Hospital on the 30th of last November, and her hand presented the appearance exhibited by the cast taken at this time. A tumour occupied the inner part of the dorsum of the left hand, extending from a little below the wrist to the junction of the fingers and the metacarpal bones. The history was, that about nine years before, she first noticed a swelling in this position, not larger than a marble. This increased very gradually at first, but being a countrywoman, constantly engaged in laborious employment, she frequently struck and injured it. Every time it was injured in this way a swelling followed, which subsided to a certain extent. Yet, by the repeated injuries it sustained, the tumour made progress in size until six months ago, when it was not more than half the size which it presented on her admission to hospital. During the last six months it increased rapidly and changed its character, according to the account given by the patient. Up to that time it was hard, and, except when injured, painless. Now it increased in size, became soft at the apex, and was greatly more painful on being handled. On examination the tumour was found to project slightly into the palm of the hand in its inner aspect, and the two metacarpal bones of the little and the ring finger could not be made out distinctly, but appeared to be enclosed by the tumour. The feel of the tumour was fluctuating at the apex, but the base was excessively hard, particularly where it passed into the metacarpal bones of the little and ring fingers. The patient had pain upon pressure, but otherwise very little suffering from the tumour. The diagnosis seemed to lie between cystic disease of the bone, expanding its walls and forming cysts which contained fluid, and an enchondromatous tumour which had softened. On the whole it seemed likely that it was the latter. On making an exploratory puncture with a small trocar a few drops of blood, mingled withoil globules, only escaped. On the 11th, Dr. Barton removed the tumour with the ring and little fingers. He found it could be readily separated from the metacarpal bones of the middle finger. Dr. Barton then exhibited the tumour and described its pathological appearances. It was a remarkably good specimen of a softened enchondroma—the lower part, where it sprang from the metacarpal bones, was formed of cartilage, which showed, upon section, the peculiar bluish colour of fetal cartilage. The upper part of the tumour, where fluctuation had been distinctly felt before its removal, was formed of a reddish, soft, granular substance, resembling sago or tapioca, coloured with red wine. Under the microscope this structure showed numerous large oil globules and nucleated cells and blood discs.—*December* 14, 1867.

Chronic Ulcer of Stomach, opening the Coronary Artery.—Cicatrices of former Ulcers.—Dr. Stokes exhibited the stomach of a patient who had

died from a chronic ulcer of the stomach, which had opened the coronary artery, and so caused death by hemorrhage. The subject of the disease was a very old woman who was admitted into the Meath Hospital some months ago. Her history was, that during the last three years she had suffered from three different attacks of great pain in the stomach. From the two first of these attacks she seemed to have perfectly recovered. She came into hospital complaining of extreme pain in the abdomen. When this was subdued she had an excellent appetite, and used an abundance of nourishing food. Without any premonitory symptoms she was suddenly seized with convulsions and died. Upon dissection the stomach was found to contain a vast quantity of blood, and a great deal had passed into the intestines also. On examining the stomach a very large ulcer was found situated upon the pancreas, the structure of the pancreas forming the basis of the ulcer, and at nearly the centre of the ulcer an artery of some size was found open. From this ulcerated arterial opening the blood had evidently escaped. Specimens of this exact termination of the same disease, Dr. Stokes observed, had on more than one occasion been exhibited to the Society, but a further interest existed in the present case in the cicatrices of two former ulcers being found, which may be fairly supposed to correspond with the different attacks of gastric pain from which this woman had suffered; and the interesting point in the case was the proof it afforded of the possibility of perfect cicatrization of ulcers in the stomach. Another point of interest was the manner of the woman's death, namely, by anemic convulsions, the same as we usually observe from any sudden and great loss of blood, such as the bursting of an aneurism or other such cause, and where the patients do not immediately die from syncope. Dr. Stokes considered that the principal interest of the specimen consisted in its proof of the reparative powers of nature in the healing of an ulcer in such an organ as the stomach.—*December* 14, 1867.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.[a]

THIRTIETH ANNUAL SESSION.

Dr. Hardy, President.

Dr. Sawyer, President for the past Session, delivered an address on resigning the Chair.—16*th November*, 1867.

Dr. M'Clintock read a paper on the spontaneous elimination of uterine tumours (printed at p. 20).—14*th December*, 1867.

[a] These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

Chorea.—The Secretary read the following communication on chorea, by Mr. Lawson Tait, of Wakefield :—

After a somewhat lengthened examination of the literary history of this disease, I have come to the conclusion that to our great English physician Sydenham is due the credit of having first accurately described it, and separated it from the many other affections with which it was confounded; and that the name by which it is generally referred to by continental writers of last century—*Chorea Sancti Viti Sydenhamiana*—is deserved. "If a glass of liquor be put into the hand to drink, before the patient can get it to his mouth he uses a thousand odd gestures; for not being able to carry it in a strait line thereto, because his hand is drawn different ways by the convulsion; as soon as it has happily reached his lips, he throws it suddenly into his mouth, and drinks it very hastily, as if he only meant to divert the spectators."[a] He limits the usual period of its occurrence to the interval between the tenth and fourteenth years. He thus describes a condition before unmentioned, and describes it as only a master could from direct observation. It is to be regretted, however, that he headed his paragraph on the affection with the title of "St. Vitus' Dance," as he has thus given rise to great confusion and no little contradiction among later writers, who confused his disease with the extraordinary state of matters then known on the continent by the various names of *Danse de St. Guy*, Krubel-Krankheit,[b] &c. That this morbid state was not that so graphically alluded to by Sydenham any one may satisfy himself by referring to the description of it in the *Anatomy of Melancholy*, which seems to have been taken from Horstius,[c] who, I think, has got it from some one else. Most writers on the subject, many of recent date, quote the description of the antics of the mad women at the chapel near Ulm and draw no distinction between them and the Chorea Sydenhamiana. Such convulsions were, I believe, nothing more than the exaggerated and epidemic hysteria which recurs now and again at intervals of several decades, in connexion with religious movements. We have had instances in this country within times not very remote in the celebrated Kilsyth and Cambuslang revivals, and in the epidemic of *Leaping-ague*, described in Sir John Sinclair's Statistical Account, and by the late Mr. Crichton of Dundee. In 1751 we have one of the ministers who fanned the flame of the disease, lamenting that of the thirty thousand who had obtained grace by a convulsion but very few had remained steadfast to their sudden conviction. In 1796 an epidemic of this kind in Anglesea,

[a] Schedula Monitoria, p. 504, Swan's edit. Bouillaud, probably from similar exhibitions, has given the affection the apt name of *folie musculaire*.

[b] Sennertus, Institutiones Medicinæ, Wittenberg, 1611.

[c] Horstius, Op. Medicin. Tom. ii., lib. viii., p. 105.

which had affected *twenty-five* women and one boy, was put an end to by the enforcement of the advice of Dr. Haygarth, of Bath, that the convulsed should be separated from all such as were at all likely to become so. Such a condition seems to have existed in small communities, more especially when shut off from more healthy excitement; and it has almost invariably been confined to the female sex. Professor Whytt has mentioned the frequent occurrence of such epidemics in the Shetland Isles, and Pennant has given interesting details of one in the island of Unst. It began by a woman being seized with convulsions in church, and spread so rapidly, and continued so long, that for years public religious services could not be conducted without interruption from this affection. Pennant naively states that it was considered by most *men* to be hysterical. In 1793, fifty years after its outbreak, it had become almost extinct, and its cure was attributed, by a clerical observer, "to a rough fellow of a kirk-officer who tossed a woman in that state, with whom he had been frequently troubled, into a ditch of water. She never was known to have the disease afterwards, and others dreaded the like treatment."[a] The more modern camp-meetings in America, and even, it is said, in some parts of Cornwall and Wales, sometimes present similar characteristics.

The *Malleatio* of Morgagni does not seem to me to describe any of the peculiarities of chorea, although many writers of authority have thought so. The patients which he and others[b] describe as constantly *hammering* with one hand on the other, or on the knee of the same side, do not seem to have been affected with Chorea Sydenhamiana. These would appear to be rather cases of brains diseased in some special manner to do special abnormal acts,[c] just as Brown-Séquard's rabbits twisted about under the operating needle; or, as Reil says, referring to eccentric movements:— "Should we not arrange those cases as slighter degrees of the same state, in which absurd ideas involuntarily occur to healthy people, incessantly incite them, and are accompanied by an irresistible propensity to action."[d] We are all familiar with the story of the poor man who was shot in a duel because he could not resist the inclination to finger the nose of another which was particularly sharp.

The *Scelotyrbe* of Galen has been generally supposed to refer to chorea, and is used evidently by Sauvages with that significance. But the

[a] Rev. W. Jack, of Northmaven, Old Stat. Ac. Similar treatment stopped the fits in the venereal wards in Paris, when the women were "recovering from their mercury."

[b] In Crawford's article on chorea (Forbes and Conolly's Encyclopædia), a case of malleatio similar to Morgagni's is mentioned.

[c] The celebrated case at Oldham, published by Mr. Kinder Wood, was of this nature—it certainly was not Chorea Sydenhamiana, neither were the extraordinary cases described by Dr. Watt of Glasgow.

[d] Fieberlehre, Vol. iv., p. 626.

original description in the Galenic writings seems to me to point to an observation of progressive spinal paralysis (*ataxie locomotrice*) much rather than of chorea; and the definition of it as a disease, "quâ erectus ambulare homo non potest," makes this almost certain. Had it been the chorea of Sydenham to which Galen referred, its frequency among children and the extreme rarity of the affection among adult males, would not have escaped his observation; besides, choraic patients have rarely any difficulty in walking or in walking erect, except in very severe cases, nor have they inability to guide themselves to the right or left, as in "*scelotyrbe.*" The term scelotyrbe seems to have been used by early writers with the most indefinite meaning. Sauvages,[a] quoting Pliny,[b] seems to refer under this term to the affection of the legs we see in cases of purpura. He says in another chapter, "Nullus est autem morbus hodie cognitus, qui ad scelotyrbem referri queat quam chorea viti Sydenhamiana."

Etiology.—Sydenham has remarked—"This disorder is a kind of convulsion which chiefly attacks children of both sexes from ten to fourteen years of age." This statement, which every author on the subject has most fully borne out, is one which does not apply to *malleatio*, *scelotyrbe*, or *danse de St. Guy* in the description of any author who mentions them. The following table shows the influence of age and sex in 501 cases.[c]

Age.	Males.	Females.	Total.
From 1 to 4	4	2	6
„ 4 „ 6	2	4	6
„ 6 „ 10	46	103	149
„ 10 „ 15	58	193	251
„ 15 „ 20	7	65	72
„ 20 „ 30	—	12	12
„ 30 „ 40	—	—	—
„ 40 „ 50	—	1	1
„ 50 „ 60	—	3	3
[d]72	1	—	1
	118	383	501

It is shown here that Sydenham was as nearly right as he could be without the statistical examination of a mass of cases. During the period he gives for its most common occurrence, 251 cases of the 501 are to be found.

[a] Nosologia Methodica, Vol. ii., p. 563. [b] Libr. xxv., cap. 3.

[c] The greater number of cases are taken from the following authorities:—Dr. Hughes, in Guy's Hospital Reports, Vol. iv.; Easleman's Statistics; Rufz on Chorea, Archives Generale de Medecine (cases in the Children's Hospital); and many scattered cases are taken from various books and papers.

[d] Case of a medical man recorded by Graves in the Medical Gazette, October, 1838.

Some authors have pointed out that different climates and seasons have different effects in the production of chorea. Dr. Bond, of Southampton, states[a] that it occurs more frequently in winter than in summer, and in northern than in tropical countries; and quotes Wicke and Hasse in support as giving 13 cases which occurred in spring, 12 in winter, 9 in autumn, and only 1 in summer.[b] He also states that the patients are generally young and females. Heberden and Elliotson agree in giving the ratio of 1 boy to 4 girls of children affected by chorea. In the table given above more than 3 girls were attacked for one boy from 10 to 15 years of age, and this proportion is very nearly the same in the mass of the cases. After 15 years of age there are 81 females to 8 males—a proportion of more than 10 to 1. Trousseau says that after 15 it is almost entirely confined to females. Some general statistics give the following results:—

	Males.	Females.	Total.
Hampshire Hospital,	6	12	18
[c]Norwich Hospital,	27	57	84
Dr. Manson, of Nottingham,	19	53	72
	52	122	174

The connexion between chorea and rheumatism is mentioned by authors of a much earlier date than is generally supposed. Hoffman[d] accurately describes chorea; and in one paragraph, somewhat obscure, seems to notice a statement of some other author, that there was some connexion between this disease and "*arthritis sic dicta vaga scorbutica.*" Morgagni[e] relates what must have been a fatal case of chorea in a young woman, aged twenty-six, while labouring under an acute disease which, I think, probably was acute rheumatism. Elsewhere[f] he records a somewhat anomalous case, strongly resembling chorea, however, in the description, occurring in a young nobleman, whose father he specially draws attention to as having suffered much from articular pains. He records an opinion expressed on this case, that there existed a spasmodic rheumatism or a convulsion from saline particles falling upon the muscles of the thorax and limbs, and these stimulating the nerves. The case terminated in death, and on *post mortem* examination spiculæ were

[a] Brit. and For. Med. Chir. Review, 1860.

[b] Rufz, on the other hand, has observed it to be more common in the hot than in the cold months.

[c] Edin. Med. and Surg. Journal, Vol. viii. Dr. Reeves.

[d] Fred. Hoffman, Opera Omnia, Genevæ, 1740, Tom. iii., Cap de motibus spasmodicis vagis.

[e] De sedibus et causis morborum, Venet. 1761, Epist. art. 5.

[f] Epist. xxv., art. 6.

found in the falx, which Valsalva, who had seen the case, accounted for by saying that the matter which goes to form gout being diverted by the improper use of mud baths, to which the patient had been subjected, had gone to form the spiculæ which caused the convulsions—an opinion which the present state of our knowledge on the subject will scarcely allow us to dispute. Sydenham's somewhat similar idea of the pathology of the disease—"This disorder appears to me to proceed from some humours thrown upon the nerves, which, by their irritation, excite preternatural motions," is about as far as we have got, now nearly two hundred years since he wrote it.

Sauvages[a] mentions a boy, aged twelve, labouring under chorea (scelotyrbe), and says, "arthridite rheumatismali recens sanatus incidit sensim in instabilitatem totius corporis." He also mentions the case of an old woman who suffered from a variety which he names *scelotyrbe festinans*, and who "laborat enim rheumatismo sicco."

Stoll[b] and Hamilton[c] both mention cases of chorea commencing with rheumatic symptoms.

The first distinct mention, however, which I have been able to find of the intimate relation of chorea and rheumatism as a well-ascertained condition, is in a *Syllabus of Lectures on Practice of Medicine*, published for the use of the students at Guy's Hospital in 1802. Unfortunately, it does not give the author's name; and although I have made many inquiries on the point (in which I have been kindly assisted by my friend Mr. Bryant), I have failed to ascertain it. In a later edition the same syllabus mentions the fact that the two affections sometimes alternate, a statement which has since been amply verified.

Trousseau has observed a connexion between scarlet fever and chorea, and the fact that acute rheumatism is one of the less frequent sequelæ of scarlet fever is well known.

Dr. Copland, in his article on chorea (*Dict. Prac. Med.*), claims to have been the first to show the connexion between chorea and rheumatism, having published a fatal case in the *London Medical Repository* for 1821, p. 23. Dr. Bright[d] first drew attention to its occurrence along with pericardial and pleuritic inflammations, and considered these the causes of the motions by the irritation from them upon the general system through the medium of the phrenic nerve—a doctrine belonging to the mechanical school of pathology, to advance which Marshall Hall attempted so much. The discovery of the connexion between the two affections is strangely attributed to Addison, as having been made by him so late as 1841, and this by no less an

[a] Nosologia Methodica, Francois Boissier de Sauvage. Amstel, 1768, Tom. i., p. 592.

[b] Ratio medendi. [c] On purgative medicines.

[d] Med. Chir. Trans., 1838.

authority than Dr. Babington. It was not till 1847 that physicians were led by Dr. Begbie[a] back to the safer, if not more satisfactory, doctrine of humoralism; that the two affections had really no relation as cause and effect, but that they really both depended on a blood-poisoning—probably the same poison acting differently under different circumstances.

The statistics of the cases of chorea as associated with rheumatism—at least those to be depended on—are of no great extent. Dr. Hughes gives the only list of any consequence, and there barely 14 per cent. are mentioned as having been directly connected with rheumatism. Other lists of less extent raise the per centage to nearly 30. My own experience is that I have not seen a case which has not been more or less intimately related to rheumatism.[b] M. Rufz is the only author who has not observed any relation between chorea and other affections, and he does not believe in its spread by imitation.

Dr. Anthony Todd Thomson, in a lecture on Chorea,[c] says—"If death occur from what may be termed *idiopathic chorea*, it is almost always the result of that form of the disease which sometimes accompanies rheumatic fever;" and this statement would certainly appear to be borne out by facts; for of twenty-six cases of chorea with a fatal termination, in which the history is satisfactorily given, I find that twenty-one—18 females and 3 males—have a distinct history of rheumatism, generally severe; and that only five—3 females and 2 males—have no mention of rheumatism in their history. Sex again seems to bear heavily on the disease, for Dr. Webster[d] states that chorea is not only much more common in girls than in boys, but that it is much more fatal in the former; and the report of the Registrar-General for the same year gives 20 females and 4 males as dying of chorea.

The *post mortem* examination of the brains in a large number of fatal cases has led to no satisfactory conclusions. In the majority of instances the appearances are nil; and in the cases where abnormal conditions are found these are so contradictory as to lead to no positive result. I have examined the records of nearly a hundred cases, and have found the results so various that it would answer no purpose to tabulate them. In nearly sixty cases no abnormality was found. Dr. Addison speaks of having had repeated opportunities of examining the bodies of patients who have died of chorea, and of never having detected any lesion of the cerebro-spinal mass, except in one case where there was chronic

[a] Edin. Med. Jour., 1847, and Begbie's Contributions to Practical Medicine, p. 68.

[b] In the investigation of this point physicians should not forget the well-known hereditary tendency of rheumatic affection. Such a tendency in chorea has been noticed by Bright, See, Hasse, and Begbie.

[c] London Medical Gazette, Nov., 1831.

[d] Lancet, December, 1840.

thickening of the *dura mater* in the course of the middle meningeal artery. He had seen many cases of meningeal inflammation going on even to pus, yet never in one did he observe choraic movements. Sir Benjamin Brodie records a case where he found a tumour about the size of a hazel nut in connexion with the pineal gland. Prichard and Bright record instances where the choroid plexus and velum interpositum were much congested, and this I have also found. Effusion into all the serous cavities, more or less abundant, has been several times found. Tumours in various parts of the cerebrum and cerebellum have been noticed. In two cases there was softening of the fornix; and in one, softening of the cerebellum. Rostan (*Medicine Clinique*, 1830) mentions a fatal case in a woman aged fifty, who from childhood had been affected by chorea of the whole left side of her body, with atrophy of the limbs; yet, contrary to his expectation, a *post mortem* examination showed nothing.

The conclusion to which we are forced by these embarrassing conditions is that the muscular affection does not depend on any local affection of the cerebro-spinal axis further than by a toxemia—"the humours thrown on the nerves"—and that whatever organic lesion there may be of nerve substance it is generally secondary to the original cause, or quite independent of the affection altogether, and but rarely is the direct cause. Mead, in his *Precepta Medica*, thus briefly dismisses the affection:—"This disease, alike ridiculous by appearance and reason, I believe to be a paralytic affection, and by frequent cold bathing and chalybeates advise to be dispelled." In his *Imperium solis ac Lunae in Corpora Humana* he expresses his belief in the influence of the luminaries in this disease. "For the most part," he says, "the disease is mild, and attacks weak ones and such as are of a tender habit of body; girls more frequently than boys, and very rarely adults."

Having found that the female sex is much more predisposed to attacks of chorea than the male, and that the period of the greatest frequency of attack is just about the time of the commencement of their specific physiological function—menstruation; it becomes a point of great importance to inquire how far the disturbance of this function influences the attack or termination of the disease. Dr. Hughes says that females are rarely affected by chorea after puberty unless they are troubled with some irregularity of the periodical functions of the uterus, or unless the disease be directly dependent on rheumatism or disease of the brain and spinal marrow. In one of his three fatal cases the disease occurred simultaneously with suppression of the menses. Davis[a] mentions a fatal case in connexion with deficient and irregular menstruation. Another fatal case occurs in the *Edin. Med. and Surg. Journal*, Vol. i., where suppressed menstruation is given as the cause. Dr. Bright, in his Reports,

[a] Obstetric Medicine.

attributes the disease in a fatal case to uterine disease. Dr. Harrison[a] gives a case where it was associated with precocious menstruation at eight years of age, and in which the attacks were evidently governed by the menstrual flow.

Dr. Hawkins[b] relates a case which occurred in the Middlesex Hospital of a girl who had violent chorea seven weeks after acute rheumatism. The catamenia had been suppressed for four months. No treatment was of any avail, and she died on the sixth day of treatment. Dr. Sunderland[c] details the case of a girl who was attacked by chorea in the beginning of January and rapidly became so bad that she had to be secured in bed. On the 6th of February the catamenia appeared and the convulsions ceased, but recurred with equal violence on the 8th, when the flow ceased, and she died on the 12th.

Bidingfield mentions, in his *Compendium*, difficult or suppressed menstruation as a special cause of chorea. Dr. Chourn[d] records the case of a girl, aged seventeen, in whom the chorea was so bad that it contined during sleep, and was so violent that the patient was wakened by the blows she gave herself. In this case menstruation had never taken place; and it is the only instance that I have found of the movements occurring during sleep. Lisfranc has recorded cases where he found the uterus engorged and menstruation irregular, and in which local depletion and iodide of potassium quite relieved the affection. Similar cases are mentioned by most writers on the disease.

The interruption of the menstrual flow by impregnation seems to exert a very special influence as an immediate cause of chorea in those already disposed to it. Dr. Prichard mentions the case of a girl of loose life, aged nineteen, who had violent chorea in connexion with pregnancy. She aborted and died. The *post mortem* examination revealed nothing but general congestion of the cerebral mass, and more especially of the choroid plexus. Dr. Peacock mentions a case in connexion with pregnancy at the sixth month. Dr. Bond mentions chorea in connexion with pregnancy, and explains it by reflex action from the fetus (an explanation which only renders the obscurity of the condition more opaque). Dr. T. King Chambers (*Clinical Lectures*, 4th edit., p. 370) gives the following interesting case:—

Sarah C., aged twenty-three, married, and full six months pregnant for the first time, was attacked with acute rheumatism and chorea at the same time. She had a cardiac complication, which subsided along with the rheumatic symptoms, but the chorea continued until the child was born at the full time, and then was cured. "Had this young woman,"

[a] Lancet, 1828.
[b] Lond. Med, and Phys. Journal, 1826.
[c] Med. Chir. Review, May, 1829.
[d] Lancet, Aug., 1839.

he says, "been in any danger it would have been worth while to have induced premature labour; I have no doubt it would have stopped the chorea, but the remedy would have been worse than the disease." Dr. Tanner has kindly favoured me with a proof-sheet of a coming edition of his work, which contained the following interesting cases:—

A Jewess had suffered from chorea as a girl, and had been successfully treated; fifteen years afterwards she married and became pregnant, when an attack of chorea set in more violent than Frank had ever witnessed in a practice of fifty years. The spasmodic movements continued day and night; and although there was perfect consciousness, yet the behaviour was most violent. The integuments were covered with boils and gangrenous spots. None of the remedies used had any effect; but a miscarriage took place at the fifth month, and health was restored at the same time.[a] Dr. Ingleby[b] narrates five cases of chorea in connexion with pregnancy, all of which were fatal; in one which the convulsions did not come on until very near the end of gestation, and did not cease after delivery. The patient died, apparently from extreme exhaustion, little more than twenty-four hours after delivery. Dr. Lever[c] narrates the case of a primapara who suffered severely from chorea after the third month of gestation until labour set in, when during uterine contraction the movements abated and recurred with their usual violence in the intervals. The disease had become so bad as to affect her intellect seriously. At the end of a month after delivery all traces of the affection had disappeared. It is also worthy of notice that the child of this patient had an attack of chorea at the age of twelve, the removal of which was simultaneous with the appearance of the catamenia, which afterwards occurred regularly. Dr. Lee[d] records a case of severe chorea where nature expelled the contents of the uterus forty-seven hours before death. The patient was in the sixth month of her second pregnancy, and had a rheumatic history. *Post mortem* examination showed nothing of consequence. Dr. Lee seems to have entertained the idea of inducing premature labour in this case. Dr. J. M. Duncan gives two mild cases in the *Edinburgh Medical Journal* for 1854, both of which recovered before confinement.

The following sentences are the notes of a case which fell under my care a short time since, and are I think of sufficient interest to be placed on record:—

Sarah T., a thin, anemic-looking blonde, aged twenty-seven, seen for the first time June 19, 1867, with well-marked, almost violent chorea.

[a] Josephus Franks, Praxeos medicæ universæ precepta. Tom. i., p. 348. Lepsiæ, 1841.

[b] Lancet, February, 1840.

[c] Guy's Hospital Reports, second series, Vol. v., p. 4.

[d] Clinical Midwifery, second edition, p. 112.

Her former history is, that when scarcely fifteen she had a severe attack of rheumatic fever, from which she recovered with choraic movements, that slowly left her in a few weeks. Ever since she has suffered from rheumatic pains. Her mother is very rheumatic, and her father slightly so. Three years ago last seventh of May she had her first child. About twelve weeks after the last appearance of the catamenia, choraic movements began, and continued with increasing severity until labour set in; never, however, so bad as to prevent her attending to her household duties. The moment the child was born, she says, the movements left her. The child of this labour, a girl, is now markedly choraic. Rather more than a year after her first child was born she became again pregnant. The movements began about the same time as in the first pregnancy, but throughout they were decidedly more severe. They likewise ceased when labour was over. A few weeks after their cessation she suffered from a severe attack of typhus fever, during which the movements were distinctly manifest. About seventeen weeks ago she states that she was aware she was pregnant, because she had not been unwell for seven weeks, and had been suffering from the movements for nearly a fortnight. They were becoming very severe, when she had an abortion, which she attributed to lifting a weight. They ceased immediately the birth was removed. She must have become pregnant again almost immediately, because she never was aware of a menstrual period after the miscarriage. She has been suffering from the movements for nearly twelve weeks, and the uterus is now the size attained between the third and fourth months. Within the last few days the movements have become much aggravated, worse, she says, than ever they were. Her appearance and motions are very characteristic of severe chorea; her tongue is much bitten, and her limbs are much bruised. She has not slept for four nights. The heart sounds are quite normal; consciousness and memory are unaffected, and so is speech, save that it is *snappy*, as is usual in chorea. She throws articles from her, but handles her child with care. The urine contains no albumen, is deficient in chlorides, and contains a very considerable amount of sugar; sp. gr. 1,031. Ordered fifteen grains of the bromide of potassium every hour.

20th.—No better; increased the bromide to a drachm every hour, but with no effect, save that the quantity of urine was increased.

21st.—Applied ice-bags to the spine and gave opium freely without effect.

22nd.—Asked my friend Dr. Atkinson to see the case in consultation. Antim. tart. gr. ii., tr. opii. ʒi., aq. ʒviii., an ounce every two hours. Persevered in till the 25th without any benefit. The pupils are quite pin-point size, while the movements have become so violent and the sleeplessness so persistent, that my proposal to empty the uterus is resolved upon with the consent of Drs. Atkinson and Holdsworth. On the forenoon

of the 26th I introduced a gum elastic catheter into the uterus, and allowed it to remain until 8 p.m., when I introduced a sponge-tent, and put the patient under chloroform. A succession of sponge-tents allowed me to turn the child, and extract it by the feet, at 6 a.m., on the morning of the 27th. No blood was lost, although I had some difficulty in removing the placenta. She was kept under chloroform until 9 a.m., when she awoke, free from the movements, and had some food. They came on again, however, about 7 p.m., and at 9 were so bad as to necessitate the re-administration of chloroform, which was kept up for nearly twenty-four hours. Being by that time utterly worn out by the case and press of other work, and lacking other means for the proper attention to the case and the administration of the anesthetic, its use had to be given up. During the night of the 28th she was so violent as to require five people constantly to hold her. About 10 a.m., on the 29th, she became comatose, and died in twelve hours. *Post mortem* examination revealed nothing, save very marked congestion of the vessels of the brain, more especially the choroid plexus, a recent clot from the rupture of a small vessel in the velum interpositum, and the dura mater rather adherent to the brain surface. The uterus was well contracted, and showed no evidence of injury from the induction of labour.

There are two points in this case on which I must express regret; first, that, with such evident indications before me, I did not induce labour at a much earlier date; and second, that the patient was allowed to become fatally comatose for want of chloroform. Should I ever meet with a similar condition again, I shall keep the patient under its influence six months rather than have a like result. The first mention which I have found of the use of chloroform in subduing the movements of severe chorea, is recorded by Mr. Harris[a] in a case where he kept up its influence for a fortnight, with a successful result. Dr. Barclay, Medical Registrar of St. George's Hospital, details a case[b] of severe chorea depending on amenorrhea, when the patient was kept anesthetic for nearly two days with a successful result. Sir James Simpson[c] recommends its employment in such cases. Dr. Gery[d] says that at the Sick Children's Hospital at Paris the use of chloroform in chorea produced the most beneficial results. Dr. Murray, of Newcastle, relates a fatal case occurring in a young lady, twenty-one, at the menstrual period. She was kept under chloroform for more than fourteen hours, with the result of arresting the movements completely; but she died forty hours after the cessation of the anesthesia, apparently from exhaustion.

[a] Lancet, June, 1848.

[b] Medical Times and Gazette, May, 1853.

[c] Obstetrical Memoirs.

[d] Bulletin Generale de Therapeutique, 1855. In the same journal a very severe case is recorded where anesthesia was kept up for four days, with a good effect.

Dr. Tanner recommends it to be used.

In regard to the further history of the treatment in the case of my poor patient, I may only say that during her many attacks she had had a multitude of counsel—that all suitable remedies had been tried, from quinine and iron down to sulphate of aniline, and all with the same effect—none. The only remedy I have ever seen of any permanent benefit in this disease is *arsenic*.

The occurrence of sugar to some extent in the urine of this patient is of interest. It has been noticed before frequently, but the observations on the point are rather desultory. Whilst one author says it almost always occurs, another will say (as Dr. Chambers tells me) that he has not been able to find it. A very general condition of the urine found in this disease, specially noticed by Walshe and Todd, is an abundant deposit of lithates. Dr. Bence Jones noticed in severe chorea that the phosphates were diminished in quantity, and that the sulphates were in great excess; and that the specific gravity might be as high as 1,036.

Since the above was written I have met with the following cases which illustrate the connexion between rheumatism and chorea, and the hereditary tendency of both; indeed I believe if the family history of every case is examined carefully it will be found that there is strong reason to believe that the two affections are really the same, under different aspects. They will also show the efficacy of a new method of treatment. The first was the case of an interesting little girl, the daughter of a medical friend. B. S ——, aged thirteen, had slight choraic movements in the beginning of January last, and got gradually worse until March, when, for three weeks, she could neither speak nor swallow for the movements of the tongue. She could be fed during that time only when flat on her back, with her head fixed, the food being placed on the back of the tongue. Almost every remedy tried had no effect, save the bromide of potass, and that only slight. The application of the ether spray down the spine had the invariable effect of sending her to sleep for an hour or two, and of mitigating the motions. To it her father attributes the cure which was accomplished in June. The family history is an interesting one. Mrs. S. had, when a child of twelve or thirteen years of age, a severe attack of chorea, which lasted many months, and this year, the forty-third of her age, she has suffered from a very severe attack of acute rheumatism. Mrs. S.'s mother had acute rheumatism in her fortieth year, and has been a martyr to the chronic affection. An elder daughter of Mrs. S., now in good health, also suffered from chorea at about the same age as B. S.

E. K., a delicate blonde, exhibited slight motions early in May, which in a few weeks became very severe, affecting speech. The menstruation had always been scanty and painful. The ether-jet had the same effect as in the case of Miss B. S. She was quite well in August.

A. F., a delicate lad, aged twelve, employed in a telegraph office, was attacked seven days ago with severe chorea. His maternal grandfather had acute rheumatism in middle life, and his mother's youngest sister had severe chorea when a child. The ether spray was applied every other day, with the effect that the morning after the third application he went to his work, and in a few days was quite well. The application was followed by the same somnolence as in the other cases.

In conclusion I may, perhaps, be allowed to draw attention to a case,[a] perhaps unique in the history of the disease, where the movements existed from birth, and probably even during fetal life. A woman when in the fourth month of pregnancy had a very loathsome object thrown at her and was ill of a low, nervous fever for two months after it, but recovered her health and went to the full time. She remarked that the child was more than usually lively in her womb, so much so as to disturb her considerably. At birth the child, a female, displayed marked chorea, which continued till the patient was thirty. The mind is hardly removed from complete idiotcy, and the patient looks little more than an elderly child. The motions are constant, save in sleep.—*December* 14*th*, 1867.

Specimen of Rupture of Uterus. By Dr. Speedy.

Mary M'Keogh, aged thirty-eight, admitted, for her third pregnancy, into the Rotundo Hospital on the 4th January, 1868. She was extremely delicate and weakly, and had lingering pains. After some few hours the head was found not to progress, and early the following morning the forceps were applied ineffectually, delivery being accomplished by means of the crotchet. There was no hemorrhage nor any recession of the presenting part; stranger still, the uterine action was strong, not only during delivery, but in the interval of ten minutes which elapsed before the placenta was expelled; her pulse became weak and rapid; the face wore an anxious expression; and coffee ground vomiting set in almost immediately. She was placed on opium and dried soda, alternately with prussic acid mixture very fourth hour; brandy, beef-tea, ice, soda-water, also nutritive enemata. There was well-marked tenderness over the entire abdomen; the vomiting still continued up to her fourth day, when it assumed a yellowish look. She gradually became weakly, and sank early that morning. On looking over the history of her former labours in this hospital, I find that the first was delivered by forceps, and still-born; the second was terminated by perforating, forceps having previously failed.

In conclusion, I would mention that there was well-marked peritonitis, the intestines being matted together with lymph, also some fluid in the peritoneal cavity. On measuring the brim I found the anterior posterior diameter to be three inches, the transverse nearly five inches. I distinctly

[a] Recorded in the Medical Gazette for May 23, 1835.

felt the promontory of the sacrum to project downwards in an oblique manner, and to this condition of the pelvis I attribute the impaction of the head in all her labours, and the difficulty of accomplishing delivery; the uterus was large and congested, showing a rupture of the cervix some two and a quarter inches in extent, situated just above the os, and to the right side.—*January 25th*, 1868.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.[a]

SESSION 1866-67.

DR. JOHNSTONE, President, in the Chair.

Aneurism of Aorta.—Dr. W. C. TOWNSEND read the following case:—John Patterson, a sailor, thirty-nine years of age, unmarried, was admitted into the South Infirmary, under the care of Dr. W. C. Townsend, on the 9th of January, 1867.

History.—Patient states that he enjoyed perfect health up to about four years ago, when in the act of heaving up the anchor of his ship he felt something give way in his left side. About two years ago he contracted syphilis, for which he took mercury extensively; no syphilitic taint at present remains in his system. During the prevalence of the severe storms, about two months ago, his ship sprung a leak, and patient slept for several nights in succession in his wet clothes, after working at the pumps. This indiscretion on his part was followed by a severe train of symptoms; he immediately complained of cough and pain in his chest, accompanied by hemoptysis; pain in chest particularly referred to left side. Having arrived in Queenstown he was immediately transferred to hospital on the above-mentioned date.

Present Symptoms.—Patient presents all the appearance of a broken down constitution—thin, pallid, and emaciated; complains of short dry cough, accompanied by sero-sanguineous expectoration in small quantity; dyspnea and loss of sleep for the past six nights. Pulse feeble, rapid, and regular; tongue clean; inability to lie on left side.

Physical Signs.—Chest expands badly on left side, which gives a dull sound on percussion over its entire extent. On applying the stethoscope no respiration whatever was carried on in any portion of left lung.

The respiration over the entire extent of right lung was perfectly healthy, and the lung on that side presented no sign of disease. Heart displaced towards left side, its apex beating in the infra-axillary region.

[a] These reports are supplied by Dr. Purcell, Secretary to the Society.

On applying the stethoscope over the region of the heart no abnormal sound was detected, but heart's action was considerably increased.

Treatment.—Ten grains of Dover's powder were prescribed on admission, and patient was put on four ounces of wine, and beef-tea diet; he obtained refreshing sleep after the Dover's powder, and expressed himself much better; an antispasmodic mixture, containing chloric ether, spt. ammon. arom., and camphor mixture was prescribed, of which he took occasional doses, together with small doses of Dover's powder, up to the time of his death, which occurred ten days after his admission, by the bursting of an aneurism.

Patient on the day of his death ate his dinner heartily, and was conversing with his fellow patients when he suddenly threw up a large quantity of blood, and expired in less than ten minutes.

Autopsy.—Twenty-four hours after death left lung atrophied and hepatized from base to apex. Pleura on left side contained over two pints of fluid, which, singularly enough, did not produce any bulging of the inter-costal spaces on that side. Surface of heart covered with a layer of fat two lines in thickness. Heart itself flabby and slightly enlarged; the outer surface of the muscular substance presenting the appearance of fatty degeneration. Hypertrophy of left ventricle; valves of heart healthy, with the exception of one of the semilunar valves of the aorta, which had a small aperture in it. A large aneurism four inches in diameter occupied the ascending and tranverse portion of the arch of the aorta, and not lined by any fibrinous deposit. The aneurism communicated with the trachea (three of whose rings were absorbed) by an opening about the size of a shilling. None of the other organs were examined.—*January* 23, 1867.

Cancer of Pylorus.—Thomas Wall, labourer, aged thirty-five, was admitted into the South Charitable Infirmary and County Hospital on 13th of May, 1867, under the care of Dr. W. C. Townsend.

About six weeks before admission had an uneasy sensation of fulness and sickness about the epigastric region, with dull pain confined to the back; these symptoms continued for about a week, after which he vomited everything he took into his stomach; was always of temperate habits; his diet before admission consisted principally of bread and tea.

Vomiting, which is constant; bowels costive; countenance shrunken and sallow; vomited about two pints of a barmy-looking fluid on the day of admission; on pouring off the barmy-looking substance, which floated on the top of the vomited matter, an opaque, yellowish-looking fluid, mixed with a large quantity of mucus, could be observed. No microscopic examination of matter vomited.

No tumour or hardness of any kind could be detected, though the abdomen was much attenuated, and a careful examination made.

He was put on arrowroot, milk, and lime water, and to have a terebinthinate enema immediately.

Vomiting still continuing; no alteration in the quantity or character of the matter vomited. The lime water appeared not to agree with him, and was stopped. He was ordered the following powders, one to be taken three times in the day:—

℞ Bismuthi albi, ʒss.
Magnesiæ (Henry), ʒi.
Morphiæ mur. gr. i.
Divide in chart. sex.

Vomiting has not abated; vomited matter has the same appearance as before; an ounce of the following mixture was ordered to be taken every six hours:—

℞ Magnesiæ carb. ʒiii.
Aquæ menth pip. ℥viii.
H. mist.

Vomiting still continuing, emaciating very rapidly; magnesia mixture and powders were discontinued, and ordered the following pills, two to be taken at four o'clock and two at night, and to have an enema of turpentine in the morning:—

℞ Hyd. sub. mur. grs. x.
Pil hydrarg. grs. x.
,, col. co. grs. x.
In pil sex.

23rd.—The pills and enema acted freely on the bowels, which gave him some relief; to continue pills, and to have two grains of solid opium at night.

25th.—Opium gave him great relief; vomiting still present, but not so frequent as formerly. He was ordered to get 1 grain of opium in the morning, one in the middle of the day, and two grains at night.

26th.—Vomiting still persists; no alteration in the character of vomited matter, but the quantity exceeds that of any former period; dull pain still present, which he refers to the epigastric region.

27th.—Sinking rapidly; has all the appearance and symptoms of collapse from cholera, except the purging; died at four o'clock.

During the time he was in hospital his diet consisted of arrowroot, milk, brandy, and wine.

Post mortem appearance twenty-four hours after death. On opening the abdomen the stomach was found to be enormously distended; on

laying open the stomach it was found to contain a large quantity of yellow viscid fluid, of a disagreeable odour; its walls were very thin. On removing the organ from its connexions, the pyloric end and the commencement of the duodenum were found to be involved in a scirrhus mass; the cardiac end of the stomach was perfectly healthy. On examining the liver it was found to be contracted, but with no trace of cancerous deposit in it. Gibson's capsule was thickened and contracted; the liver presented no other aspect of disease; all the other organs of the body were healthy.—*November* 13, 1867.

Cancer of Kidney and Lung.—James Neil, aged thirty-four, was admitted into the South Infirmary, Cork, November 5th, 1867, under the care of E. R. Townsend, jun., M.D.

History.—Three months before admission got a severe cold, with great pain in the right side of the chest, and spat some blood. For this he was treated in the Macroom Union Hospital, where he remained for two months, and left it somewhat relieved; was always of temperate habits; had resided in America for three years, where he enjoyed very good health.

Symptoms on Admission.—Complains of cough and great difficulty of breathing; expectoration viscid, and tinged with blood; decubitis on the back; complete anorexia; sleeps badly; there is incipient clubbing of the fingers; urine loaded with lithates, but abundant; pulse rapid (100) and feeble.

Physical Signs.—Right side of chest acts badly; does not expand at all; left side acts well; the entire right side is dull on percussion, both anteriorly and posteriorly; left side sounds well. There was tubular breathing in the right infra-clavicular region, and as far as the fourth rib, with absence of respiration lower down, and well-marked bronchophony posteriorly. Over the root of the lung there was tubular breathing and increased resonance of voice, not amounting to ægophonism; there was exaggerated respiration on the left side; heart sounds natural.

Treatment.—He was ordered five grains of Dover's powder, and three of hydrarg. cum creta twice a day; to have a cough mixture containing lac ammoniacum, chloric ether and paregoric; the entire right side to be painted with strong tincture of iodine; to have beef-tea and four ounces of wine.

Nov. 10.—Feels somewhat better; says he can breathe easier; expectoration less viscid; no tinge of blood; is, in fact, only bronchial; to stop the hydrarg. cum creta, not wishing to salivate him; to take five grains of Dover's powder at night, and five grains of iodide of potassium in bark three times in the day.

15th.—To have a draught containing twelve minims of Battley at night instead of the Dover's powder. He continued this treatment up to

the 24th Nov., when he had an attack of hematuria very profuse; it came on during the night, and he was not conscious of it until the morning. The quantity of blood passed must have been considerable. He was ordered a mixture containing gallic acid and opium which soon checked it. He became rapidly weaker from this out, and although well supported, sank on the evening of the 29th of exhaustion, his mental faculties remaining clear to the very last.

Post mortem examination.—Right kidney greatly enlarged; weight 15 oz., presenting the appearance of a large tumour; on section the upper part was converted into an immense mass of encephaloid cancer, springing from the convex edge. There was no appearance of kidney structure in this mass. On examination there appeared to be two forms of cancer cells present, those of the scirrhus form occupying a small central portion, whilst the rest seemed to be of the encephaloid variety. The lower portion of the tumour presented the natural appearance of kidney structure, but was much congested; the left kidney was enlarged, and very much congested; it weighed 6 oz., but was healthy in appearance. No appearance of cancer in the liver, which was natural in size and appearance, with the exception of one or two patches of ecchymosis under the capsule. The left lung was studded with cancerous nodules of the encephaloid form of cancer, varying in size from a pea to that of a marble. The right pleura contained a large quantity of fluid; was very adherent in some parts, the membrane being greatly thickened, the result of the first attack of pleurisy, but with evident manifestation of recent inflammation, with freshly deposited lymph, which was quite soft. The lung had been carnified, with some evidence of exudation into it; there were some miliary tubercles scattered through it; no trace of vomicæ or broken down tubercular matter, and no cancerous nodules in it; the other organs were healthy. Dr. Johnson, in his work on the kidney, says:—"Cancer is one of the rarest forms of renal disease. In a certain number of cases it has been found to affect the kidneys alone, but in the greater number of instances cancer of the kidney is associated with a similar disease in various other parts, in the lumbar and messenteric glands, the liver, the lungs, &c. In some cases M. Rayer has found that the cancer has extended from some contiguous organ to the kidney; from the liver to the right kidney, and from the descending colon on the contiguous portion of the stomach to the left kidney. With respect to the form of cancer, the encephaloid variety is by far the most frequent in the kidney. Dr. Walsh says that in forty cases of renal cancer that he has collected, thirty-one were examples of pure encephaloid cancer or one of its varieties; in five only was scirrhus observed, whilst there was no instance of colloid cancer in this organ. The hematoid variety of encephaloid cancer is more frequent in the kidney than in most other internal organs, blood in variable proportions being mingled with the

encephaloid matter. In thirty-five of the forty cases recorded by Dr. Walsh the disease affected both organs sixteen times, the right kidney alone in thirteen cases, the left kidney alone in only six cases. He also says that while no period of life is exempt from cancer of the kidney, persons of advanced years are much the most liable to the disease. He also says the disease is more common in males than females. This man was aged thirty-four years. This man presented no symptoms of renal disease until three days before his death, when he passed a large quantity of blood. He stated that he had never before passed any blood, nor was he aware of any uneasiness in the loins; in fact, there was not the slightest symptom to direct attention to the kidneys.—*December* 11, 1867.

CLINICAL RECORDS.

Hospital Notes. By T. H. Babington, M.D., T.C.D.; M.R.I.A.; Surgeon Londonderry County Infirmary.

LONDONDERRY CITY AND COUNTY INFIRMARY, AND CITY FEVER HOSPITAL.

REPORT FOR THE YEAR 1867.

Numerical Return of Patients Admitted into and Treated in the City and County Londonderry Infirmary for the Year 1867.

Patients Remaining 31st December, 1866,	46	
Admitted,	612	658
Discharged Cured and Relieved,	533	
„ Died,	33	
„ Irregular,	5	
„ at own desire,	22	593
Remaining 1st January, 1868,		65

City Fever Hospital Report for 1867.

Patients Remaining 31st December, 1866,	23	
Admitted,	80	103
Discharged Cured,	88	
Died,	9	97
Remaining 1st January, 1868,		6

Number of Beds in Hospital, -	Males, 48 ; Females, 24—Total, 72
Number of Beds in Fever Hospital, - - - - - - -	32
Total number of days passed by Patients in Hospital, - -	7,741
,, ,, ,, in Fever Hospital, -	2,218

Numerical Abstract of Cases of Accidents, Diseases, and Deaths, for the Year 1867.

ACCIDENTS.	NO.	ACCIDENTS.	NO.
Burns and Scalds, - - -	14	Fracture of Leg, - - -	4
Bite of Dog, - - -	1	,, Compound of Leg, -	1
Compound dislocation of Ankle, -	1	,, Radius, - -	14
Dislocation of Hip, - - -	2	,, Ribs, - -	3
,, Shoulder, - - -	4	,, Thigh, - - -	2
,, Thumb, - - -	3	,, Ulna, - - -	2
Drowning, - - - -	1	Injuries of Head, - - -	12
Fracture of Clavicle, - -	12	,, Spine, - - -	5
,, Fingers, - -	7	Wounds, Contusions, and General Injuries, - - -	66
,, Humerus, - -	6		

DISEASES.	NO.	DISEASES.	NO.
Abscess, - - - -	13	Diseases of Urinary Organs, -	8
Albuminuria, - - -	6	Dropsy, - - - -	6
Anemia, - - - -	2	Erysipelas, - - - -	3
Anthrax, - - - -	3	Hernia, - - - -	4
Cancer of Breast, - - -	4	Hemorrhoids, - - -	2
,, Eyelid, - - -	1	Hydrocele, - - - -	5
,, Face, - - -	1	Nevus, - - - -	1
,, Lip, - - -	7	Onychia, - - - -	1
Cynanche Tonsillaris, - -	3	Paronychia, - - -	3
Diseases of Brain and Nervous System, - -	14	Pyemia, - - - -	1
,, Bones and Joints, -	24	Rheumatism, - - -	55
,, Eyes and Ear, -	21	Struma, - - - -	4
,, Heart, - -	10	Syphilis, - - - -	13
,, Lungs, - -	96	Tumours, - - - -	3
,, Stomach and Liver, -	50	Ulcers, - - - -	46
,, Skin, - -	23	General Debility and other Diseases,	15
,, Uterus, - -	4	Fever, - - - -	80

Diseases and Accidents which proved fatal.

	NO.		NO.
Albuminuria, - - -	1	Disease of Brain, - - -	3
Accident, - - - -	9	,, Heart, - - -	2
Bronchitis, - - -	1	Phthisis, - - - -	8
Diarrhea, - - - -	1	Pyemia, - - - -	1
Dropsy, - - - -	1	Tetanus, - - - -	1
Fever, - - - -	9	Old Age and Debility, - -	5

Operations.

	NO.		No.
Amputations of Toes, - -	3	Removal of Cancerous Lip, -	4
,, Thigh, - -	2	,, Tumour from Scalp, -	1
		,, Nevus, - - -	1

I.—*Burns and Scalds.*—Two fatal cases of burns were recorded during the year; both occurred in epileptic patients, a formidable complication in any case of accident.

One very severe case of scald of mouth and larynx in a child aged seven years, has been recently under treatment; the child inhaled the steam from the spout of a boiling kettle. There was great congestion and difficulty of breathing, the symptoms not being, in my mind, sufficiently urgent to warrant tracheotomy, and, impressed with the feeling of the unsatisfactory result so frequently following the operation, I treated the child with antiphlogistics and mercury, as recommended by my esteemed friend Dr. Bevan, in the 29th volume of the *Dublin Quarterly Journal of Medical Science*, and with the happiest effects.

II.—*Compound Dislocation of Ankle Joint.*—A young woman, aged twenty-six, cook in an hotel, slipped and fell; her left foot turned under her in some indescribable manner; the fibula was split and fractured, the lower end of the tibia was dislocated, and protruded through the skin about two inches, and a portion of the internal malleolus was detached. The patient was placed under chloroform, but all attempts at reduction having failed, I was obliged to saw off more than an inch of the protruding tibia before I could adjust the lacerated joint. A great deal of constitutional irritation followed; the patient had three successive attacks of phlegmonoid inflammation, extending to above the knee, along the thigh; many abscesses were opened, and the patient was often in such peril that I almost wished I had amputated the leg the day she came into hospital, but I had the satisfaction of discharging her after being eight months in hospital, with what will be a *very useful if not a very shapely limb.*

III.—*Hemorrhage from a wounded Radial Artery* was effectually controlled by acupressure.

IV.—*Twelve Cases of Injury of Head* came under treatment, two proved fatal; one fell into the hold of a ship, on his head, and fractured the frontal bone. He was admitted in a state of insensibility, and died whilst I was in the act of removing the depressed and smashed pieces of bone. The other died insensible two hours after admission.

V.—*Five Cases of Injury of Spine* were admitted; three proved fatal. The first lived only three days after receipt of injury; he died from effusion into bronchial tubes and air cells; the third cervical vertebra was fractured. The second fell down some high rocks, and was injured at the tenth dorsal vertebra; he lived fourteen days. The third was struck by the shaft of a cart, *as he said*, between the shoulders; he lived twenty-one days. These died from bed-sores and prostration.

VI.—*The General Injuries* include wounds, sprains, and contusions; some were admitted of a grave and severe nature; two proved fatal. A number of men were engaged pulling down a rotten old building;

the flooring gave way, pulling some walls with it, and three were buried in the ruins. One had his left thigh fractured, his right hip dislocated, and a fracture of the ileum on the same side, a wound communicating with the fracture. He died seven hours after the accident. The second was completely smashed about the chest, and lived one hour longer than his comrade. The third escaped with a few contusions. The nine fatal accidents were as follows:—Two from burns, two from injuries of head, three from injuries of spine, and two from general injuries.

VII.—*Cancer.*—In the report of cases treated in this infirmary in 1866, I enumerated two cases of cancer of the breast, removed by amputation; both considered what are called "promising cases;" the diseased structure appeared to be localized; no cutaneous or glandular contamination, and good constitution. The breasts were removed, the wounds healed well and kindly, but in six months, in each case, the cicatrix became hard, speedily ulcerated; axillary glands and skin became infiltrated; much pain and suffering; œdema of arms; and within twelve months from the operation, I followed both "promising cases" to the grave.

Four cases of cancer were received this year; in three the axillary glands were diseased, and other general symptoms were unfavourable; therefore, removal of the breast was not proposed. *Acetic acid* was injected in three cases. Two would not remain under observation. The third I see frequently; she is young, aged twenty-six; has been recently confined; about half the tumour has sloughed away. I will inject the acetic acid again and watch the result.

The fourth case is rather anomalous. A young woman, aged twenty-six, while nursing her fourth child, felt uneasiness in her right breast; the nipple became retracted to almost underneath the skin; all secretion of milk ceased in the breast; the skin all over the gland became in a similar way contracted; the gland absorbed and the skin shrivelled, corrugated, and, as it were, dragged inwards, and puckered all over the region of the mammary gland. There was much stinging pain; the patient was bright, cheerful, and most anxious to have something done for her before coming under my observation. "*A cancer curer*" had circumscribed the diseased skin with a plaster which had ulcerated the parts rather deeply. The only operation to be performed was to dissect off the whole mass, leaving it to heal as it might. I hesitated; I waited and watched; while under my observation the other breast, in which there was still milk, became attacked in similar manner. She then gave up all hope and went home. She died three months afterwards of some very acute affection of the lungs, as I heard. I am not in a position to pronounce any opinion as to the advisability, or otherwise, of operating in cases of cancer of the breast. Figures and cases can be calculated, and statistics arranged to meet every or any view; but the words of (that master mind in Irish surgery) the

late Mr. Colles, in his lectures, should not be forgotten. "For my own part it is an operation I would not press on the patient or her friends at all. It is one by which very little service is rendered at any time."

On the Rhythm of the Sounds of the Heart. By F. C. DONDERS, M.D., F.R.S.; Professor of Physiology and Ophthalmology in the University of Utrecht. Translated from the "Nederlandsch Archief voor Genees en Natuurkunde," Utrecht, 1865. By WILLIAM DANIEL MOORE, M.D., Dub. et Cantab., M.R.I.A., Honorary L.K.Q.C.P.I., Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Secretary for Sweden, Norway, and Denmark, to the Epidemiological Society of London.

I. *Introduction.*—The sounds of the heart are distinguished as first, *I;* and as second, *II.* The first begins nearly with the impulse and continues until the second falls in as a short stroke; hereupon follows the pause, which is terminated by the first sound of the following period.

The distance between the commencement of *I* and that of *II* we call α, that between *II* and *I*, *A*. Thus the duration of one period is $P = \alpha + A$.

P is known by the frequency of the impulses. If we now determine α or *A*, we know both, and therefore at the same time $\alpha : A$ and $\alpha : P$. This includes the knowledge of the rhythm of the heart.

Respecting the rhythm, little is to be met with in literature.

Laennec[a], the founder of the science of auscultation, described the sounds very well, but said nothing directly about the rhythm. It would appear, however, that he did not consider α to be much less than *A*. He had, as we know, an incorrect idea of the sequence in the action of the heart, and when he says: "Sur la durée totale du temps dans lequel se suivent les contractions successives des diverses parties du cœur.....*la moitié ou à peu près* est remplie par la systole des ventricules," this means that *I* and *II* lie at little less than half a period from each other.

The only person, so far as I know, who has endeavoured experimentally to solve the question is Volkmann[b]. His method consisted in regulating the length of a pendulum, so that the duration of its vibrations was equal to α, therefore also to *A*. At the same time he determined the duration of the whole period, and as he thus found *P* about $= A +$, [c] he thought that the correctness of his results was beyond doubt. Absolute values Volkmann does not give. He merely states, that as the mean of nine

[a] De l'auscultation médiate. Paris, 1819 (I used Andral's edition), Bruxelles, 1837.
[b] Zeitschrift f. rat. Medicin. 1842. Bd. III. p. 321.
[c] The difference stated by Volkmann is indeed rather great.

experiments, he found $a : A = 96 : 100$. Now this result appears to confirm Laennec's opinion which assumes for a, "la moitié ou à peu près.

Many years ago, however,[a] in repeating Volkmann's experiments, I found that this result does not deserve credit: it appears impossible to say of a single vibration, that it is equal to a or to A; and for a series of isochronous strokes, which would be necessary for this, the opportunity, with the constantly alternating cardiac sounds, does not exist. Indeed, I thought I had satisfied myself, that the difference between a and A, in ordinary cases, is greater than Volkmann had found it; but my results also were too uncertain, to enable me to say anything more upon the subject.

Others have, from simple estimation, thought they might assume that the heart beats in a triple measure, of which a would occupy only one-third, while the second sound with the pause would amount to two-thirds[b]. Spring, the profound writer of the *Mémoire sur les mouvements du cœur*[c], thinks he has observed, that in youth and in a state of perfect rest the heart beats in a quadruple measure, of which half belongs to the diastole, the other half to the systole with the præsystole assumed by him. But so soon as the circulation is a little accelerated, even if it be only in consequence of a change in position or of mental emotion, it becomes triple, two-thirds belonging to the state of action, and only one to that of rest. In Marey[d] we find not much more than that with respect to the rhythm of the heart a great difference exists. In general, the rhythm is not independently investigated, but deduced from, or connected with, the sequence in the action of the heart, with the phases of which the sounds have at the same time been treated.

Nor could the coryphæi of physical diagnosis in Germany satisfy me. Here they stumble on a confusion between the duration of the proper pauses (stilte), which it is certainly very difficult to determine, and the distance from the beginning of the one to the beginning of the second sound, in which we are concerned. I spoke of pauses, in the plural: some writers, in fact, assume—I mean since Purkinje—two pauses, a great one between *II* and *I*, a short one between *I* and *II*, the latter especially perceptible in auscultating over the mouths of the arteries. But sometimes, according to Skoda[e], the pause between *I* and *II* is "so short, that the second sound over the ventricles appears as it were the accented end of the first sound, *and the first sound over the arteries*" (where it is less distinct)[f] "*as it were only a premonitory stroke of the second sound.* But in

[a] Handleiding der natuurkunde van den Mensch [Guide to the Natural History of Man]. 1853. Part II. p. 44.

[b] Conf. Beau, Traité d'auscultation, p. 229.

[c] Mémoires de l'académie royale des sciences, des lettres et des beaux arts de Belgique. Bruxelles, 1861. T. XXXIII. p. 51.

[d] Physiologie médicale de la circulation du sang. Paris, 1863, pp. 105 et seq.

[e] Abhandlung über Perkussion und Auscultation. 6te Aufl. Wien, 1864, p. 207.

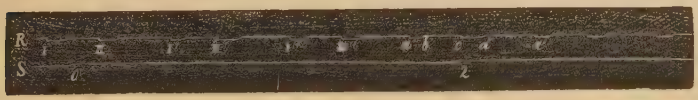
[f] Conf. Rapp. Zeitschrift f. rat. Med. 1849. B. VIII. p. 151.

other cases," continues Skoda, "the pause between the first and second sound is almost as long, or exactly as long as the pause between the second and the ensuing first sound." The words printed in italics would lead us to believe, that he is here speaking of α and therefore also of A, if all the rest had not evidently reference to the pauses alone. With Friedreich[a] I found the same confusion, even including the brief statement of Volkmann's results, which nevertheless referred only to α and A.

From all this it appears, that little trouble has been taken with respect to the rhythm of the sounds of the heart. And yet this rhythm is unmistakeably of great physiological importance. In a subsequent article I shall show more precisely, that α represents the active period of the heart, consequently the duration of the process, the ground of which is to be sought in the heart itself, while the period P is determined rather by the regulating nervous system external to the heart. In modification of the heart's action, under this or that influence, the altered frequency of the strokes is thus only one side of the question; and if our principal object be also to determine the modification of the active process, this can be known from the rhythm of the cardiac sounds. For this the simple statement of the existence of this or that rhythm is not sufficient; *it must be measured.*

II. *Method.*—In order to measure the rhythm, the cardiac sounds must be registered: that is, the times must be reduced to long measure. If with the slight force of the cardiac sounds it was scarcely to be conceived that they could be recorded by means of a body brought into a state of corresponding vibration, this might be effected in an indirect method. Now the latter was found in the registration of movements of the hand, practised in unison with the rhythm observed by the ear: during auscultation we follow the rhythm of the cardiac sounds by striking on the arm of a marker, and this registers the times on a revolving cylinder. In order in each experiment to ascertain also the frequency of the pulsations, the seconds of a clock are, by means of electromagnetism, registered on the same cylinder. We thus obtain figures, as the following:

FIG. 1.



R is the tracing of the rhythm, S that of the seconds, the distance from 0 to 2 being equal to two seconds. In the tracing R the distance $I\ II = \alpha$, and $II\ I = A$, so that $I\ I'$ as well as $II\ II' = P$. At α the marker

[a] Handb. der bijzondere pathologie en therapie, onder redactie van Virchow. D. V. Afd. 2. bl. 199.

is pushed down on hearing *I*, at *b* the finger is again taken up and the marker rises; at *c* it is again depressed on hearing *II*, it rises again at *d*, to begin at *á*, on hearing *I'*, the second period. Such periods were registered to the number of about twenty consecutively, at which the cylinder stopped: the number of corresponding seconds showed the frequency of the pulsations, and from the α and *P* measured for each period the averages were calculated for the whole circuit.

Subsequently I found, that, with regular pulsations, the rhythm is capable of being imitated with more certainty and accuracy, by omitting one after each registered period. We then write, it is true, opposite a following period the rhythm of the preceding one; but where the rhythm is regular, this comes to the same thing and has no influence on the average. We thus obtain such figures as the subjoined:

Fig. 2.

In the tracing *R'* only *A*, in the tracing *R* only α, is alternately registered, consecutively to each other from the same heart. In *R'*, therefore, between *I'* and *II''*, *II'* and *I''* have been omitted, and *II II''*, or *I' I'''* is equal to 2*P*. In the tracing *R*, between *II* and *I''*, *I'* and *II'* have been omitted, and in this case *I I''*, just like *II II''*, is again equal to 2*P*. In the tracings *S'* and *S* we again see the seconds which show the duration of the periods, and if we now measure the registered or α *A*, we have only to divide the ascertained value by the half of the like measured 2*P*, to find α or *A* in percentages of *P*.

III. *Testing of the Method.*—In order to ascertain what degree of accuracy is attainable by the method just described, we must inquire, with what precision we can reproduce, by movement of the hand, a rhythm perceived by the ear, even when the alternating distances do not stand in simple relation to one another. Besides, I was anxious, for reasons which shall be stated hereafter, to investigate how far the strokes of the hand are isochronous with those ascertained by the ear.

To fulfil both these requirements, sounds must be produced artificially in a rhythm, about corresponding to that of the cardiac sounds, and this rhythm must register itself, and at the same time be imitated in the manner above described. If the experiment be so arranged, that the markers of the primary rhythm and of that obtained by imitation stand directly under one another, the difference between the two tracings strike

the eye immediately and may now be still more accurately measured. The primary rhythm I obtained with the aid of Maelzel's metronome. By shifting the weight on the pendulum we can regulate the number of strokes at will, and by inclining the instrument, we can make the strokes alternately unequal. These strokes themselves are not, however, to be used, for the moments of origin are not to be registered; but I attached to the pendulum a wire in a horizontal direction, bent the two extremities left and right downwards, and in the vibrations of the pendulum caused these bent extremities to dip into mercurial troughs, whereby a galvanic current was at the same time closed. To insure the regular action of the instrument, it now appeared to be even better not to incline it, but to produce the alternate difference merely by altering the length of the wire, and by placing the one mercurial trough higher than the other. Now on each closing of the current, the arm of a marker was attracted by an electro-magnet, in a distant room, where the strokes of the metronome could not be heard, and the moments of attraction, with alternately greater and shorter pauses, were here registered on the revolving cylinder of the kymographion. As we heard in the strokes of the markers upon the magnet the registered rhythm, we had only to imitate this with the hand, in the mode above described—by striking, in fact, upon a marker, which registered its movements directly above those produced by the magnet. In this manner we obtain figures like the subjoined:

Fig. 3.

The tracing *M* is the primary rhythm, derived from the metronome; *J* is the imitated rhythm. In both tracings three whole periods are to be seen: *I II* represents $_a$, *II I* is *A*, &c.; at *a* the markers descend, at *b* they are set free and ascend by their elasticity, at *c* they descend anew, to rise again at *d*, and at *a'* a second period begins.

It here appears distinctly, that in the imitated rhythm *I* occurs somewhat too late and *II* often rather too early, so that $_a$ is reproduced too small and *A* too great.

The same occurs when, as in fig. 4,

Fig. 4.

a or A is alterately reproduced: in the upper tracings the A's here imitated, especially the second, are too great, because II occurs too soon, I on the contrary scarcely too soon or even too late—as in the lower tracings the a' are too small, because I occurs too late and II too early.

We have therefore here a rather considerable personal error, which with me returns each time, modified however by the values of P and of $a : P$. Of these results, I here append more than is absolutely necessary for estimating the degree of accuracy attained in registering the rhythm of the cardiac sounds. I do so because the question, what accuracy is attainable, is in a psycho-physical point of view, in itself not without importance, and moreover the method of the indirect registration of sounds will, in other cases also, find application. We should therefore know what it is capable of.

Now, in the first place, the observations, which belong to one and the same circuit of the cylinder, present a slight probable error.

Some examples may illustrate this.

No.	METRONOME		IMITATION	
	Period $P = {}_{3}\!\!\downarrow_{2}'$	a	Period P'	a'
1	12·9	4·4	12·6	4·5
2	18·5	6·8	18·7	7·4
3	21·	8·	21·3	8·4
4	24·2	10.	24·3	9·8
5	25·8	10·3	25·5	9·9
6	26·	10·5	25·9	11·
7	26·6	10·6	27·2	10·6
8	27·1	11·	27·3	10·6
9	27·4	11·3	26·7	10·3
10	27·6	12·	28·4	11·8
11	28·3	11·8	27·8	10·9
12	27·4	11·5	27·1	10·7
13	27·2	11·5	26·6	10·5
14	28·3	12·7	28·	10·8
15	26·5	10·7	27·8	11·1
16	26·4	11·	26·6	11·5
17	26·9	11·	24·7	9·5
18	26·4	10·4	27·2	10·7
19	26·4	10·9	26·4	10·9
20	26·8	10·9	26·8	11·2
21	27·3	10·3	27·6	11·2
22	27·4	10·9	26·3	11·
23	26·8	10·7	27·4	11·6
	536·3	220·	535·1	215·6

The first three observations, in which the cylinder had not yet nearly attained its full rapidity, are neglected: soon, as appears from the period of the metronome, its course became regular enough, and moreover, α adapted itself to P, so that, for the strokes of the metronome, the average result is very accurate.

In the case to which the above table belongs, all strokes of the metronome were imitated, and consequently both α and A (method a' conf. fig. 3); but only P and α are taken into the Table, because $A=P-\alpha$ and is therefore ascertained by subtraction. The difference between the sum of the periods 536·8—535·1=1·7 places us in a position to judge of the accuracy of the measurement, which indeed seems to be satisfactory.

The calculation is now as follows:

Metr. $\alpha=220$: 536·8=41% of P ($A=59\%$).
Imit. $\alpha'=215{\cdot}6$: 535·1=40·3% of P' ($A'=59{\cdot}7\%$).

The probable error in the 215·6 found is certainly so slight, that we may, with a safe conscience, save ourselves the trouble of the calculation (not the ascertained values of α', between which all the difference is, but α' : P', for each observation, should be the basis of the calculation). Yet there appears to be a personal error of 0·7 of the percentages found, and sometimes this is much greater.

A second example, in which α' alone was alternately determined (method *b*, conf. fig. 4):

No.	METRONOME		IMITATION	
	$2P=\frac{1}{31}'$	α	$2P'$	α'
3	47·2	10·8	47·2	10·9
4	47·2	10·6	46·8	11·
5	47·3	10·9	47·2	11·4
6	46·9	10·7	47·4	11·
7	45·7	10·4	45·4	10·4
8	44·	10·2	44·4	10·3
9	44·7	10·	44·2	10·6
10	45·2	10·2	45·6	10.8
11	44·4	10·	45·	11·
12	45·	10·1	45·6	10·6
	457·6	103·9	458·8	108

Here the same remarks hold good as above. The percentages of α and α' are now found by dividing by the *half* of the ascertained *double* periods. Consequently:

$\alpha = 104{\cdot}9 : 228{\cdot}8 = 45{\cdot}4^0/_0$ of P $(A = 54{\cdot}6^0/_0)$.
$\alpha' = 108 : 229{\cdot}4 = 47{\cdot}1^0/_0$ of P' $(A' = 52{\cdot}9^0/_0)$.

By a very small probable error, a difference of 1·7 of the percentages is here found, which again is to be ascribed to a personal error: that α' is above found too low, and here too great, is probably connected with the different values of P and $\alpha : P$ for the two cases.

Lastly, I add an example in which A' is determined (conf. fig. 4, $J'M'$), in which therefore the distance between II and I was alternately imitated (method *c*).

No.	METRONOME		IMITATION
	$2P = \frac{1}{45}'$	A	A'
3	37·2	10·5	10·2
4	40·2	11·8	10·4
5	42·7	12·5	12·5
6	44·3	13·5	13·2
7	43·2	13·5	12·
8	43·	12·6	12·4
9	44·	13·3	13·4
10	44·3	13·3	14·5
11	43·5	13·	13·5
12	44·7	13·6	14·5
13	43·2	13·4	13·
14	42·5	13·3	13·7
	513·4	154·3	153·3

As $A : P$ and $A' : P$ we find the percentage values of A and A', and by subtracting the quotients obtained from 100, those of α' and α, which last, even when A' had been determined, are, to facilitate inspection, taken into the Tables.[a] The calculation in this case yields:

$A = 154{\cdot}3 : 256{\cdot}7 = 60{\cdot}1^0/_0$ of P $(\alpha = 39{\cdot}9^0/_0$ of $P)$.
$A' = 153{\cdot}3 : 256{\cdot}7 = 59{\cdot}7^0/_0$ of P $(\alpha' = 40{\cdot}3^0/_0$ of $P)$.

These three examples are sufficient to illustrate the method of observation

[a] All the observations and the great majority of the measurements occurring in this investigation have been made by myself. In the few series in which I intrusted the measurements to others, I had, in the equality of the sum of the periods P and P', a test of the accuracy of their work. When I myself made the measurements, it was sufficient to determine either P or P', after I had repeatedly found that with me the results did not differ.

and calculation. I now add what was found according to the three modes of observation, with different values of P and $\alpha : P$, as α'. Of the first series I communicate all the revolutions, with a statement of the number of observations in each revolution.

Series I. $P = \frac{1}{3}\frac{1}{2}'$ (52 double strokes in the minute); $\alpha : P$ about equal $\frac{2}{3}$ (from 38·9 to 43·1%).

No.	α (Metronome)	α', obtained by			$\alpha - \alpha'$	Number of Observations
		method a (imit. of $\alpha + A$)	method b (imit. of α)	method c (imit. of A)		
1	39·9	39·			0·9	13
2	38·9	38·2			0·7	14
3	41·8	39·5			2·3	18
4	41·5	39·2			2·3	20
5	43·1	42·7			0·4	21
6	42·2	39·2			3·	19
7	41·	34·8			6·2	24
8	41·2	36·8			4·4	20
9	41·4	40·7			0·7	20
10	41·	40·3			0·7	20
11	40·7		38·8		1·9	11
12	40·9		40·1		0·8	12
13	41·		38·9		2·1	8
14	40·8		38·7		2·1	12
15	41·5		39·		2·5	12
16	41·4			39·	2·4	12
17	41·6			41·5	0·1	11
18	41·3			40·6	0·7	12
a	41·2	39·1			2·1	189
b	41·		39·1		1·9	55
c	41·4			40·4	1·	35

The calculated final averages for each of the three modes of observation are here introduced as a, b, and c: α' here appears in all the observations, and consequently also in the final averages, to be less than α. I have chosen the series in which P is greatest, although the deviations in this prove to be much greater than in most others. By the method α we can imagine the accent on either the sound *I* or *II*, at pleasure; in making the experiments this was distinguished, but it yielded no constant differences, so that to simplify the Table I have comprised all the observations, performed according to the method a, in the same column.

Series of observations, similar to that here communicated, were now made also with other values of P and $\alpha : P$. In general they appear much

better than the above. But for the sake of conciseness, we must confine ourselves to a statement of the final averages, which are collected in the subjoined table:

Series	$P=1'$:	Method	α Metronome	α' Imitation	$\alpha-\alpha'$	Number of Observations
I.	52	*a*	41·2	39·1	2·1	189
	52	*b*	41·	39·1	1·9	55
	52	*c*	41·4	40·4	1·	35
II.	66	*b*	44·1	44·2	−0·1	8
	66	*c*	44·9	41·	3·9	11
III.	65	*a*	46·5	45·5	1.	44
	64	*b*	46.	45·6	0·4	60
	64	*c*	46·9	45·1	1·8	61
IV.	72	*a*	41·4	39·2	2·2	95
	72	*b*	40·	39·8	0·2	35
	72	*c*	38·8	37·7	1·1	34
V.	73	*b*	41·8	41·5	0·3	18
	72	*c*	39·2	39·3	−0·1	9
VI.	73	*b*	46·4	46·8	−0·4	9
	73	*c*	45·7	45·6	−0·1	9
VII.	82	*b*	46·3	45·6	0·7	9
	82	*c*	47·2	48·9	−1·7	10
VIII.	83	*b*	42·3	41·8	0·5	9
IX.	85	*b*	34·2	38·5	−4·3	31
	85	*c*	33·7	35·1	−1·4	31
X.	86	*a*	39·	36·6	2·4	53
	86	*b*	39·3	41·8	−2·5	54
	86	*c*	39·9	38·7	1·2	43
XI.	84	*b*	43·9	44·8	−0·9	35
	84	*c*	43·6	42·6	1·	33
XII.	94	*c*	44·4	43·5	0·9	11
XIII.	98	*b*	38·2	37·2	1·	13
XIV.	97	*b*	40·3	40·7	−0·4	11
	97	*c*	42·5	42·5	0·	7

This table, deduced from not less than 1,022 observations, exhibits the degree of accuracy I attained to in imitating the rhythm. On an average the error amounts to 1·25 in the percentages. At first, experimenting with slight frequency of the strokes, I usually reproduced α too low, A, too high. Subsequently I found that not unfrequently the reverse takes place: in three examples α' is twice too low and once too high, with about the same average error of 1·25. The greatest deviations occur when α is comparatively little or else very great and nearly equal to A. No regularity is, however, to be traced in this matter. In order with every value of P and of $\alpha : P$ to determine the personal error, it would be

necessary to experiment and measure for many months. For our object it is sufficient to know, that on an average a personal error of 1·25 of the percentages attaches to our observations of the rhythm of the pulsations of the heart hereto appended, and that this error will not easily increase to $2.5^0/_0$. Moreover, when the P' and $\alpha : P'$ found for the heart correspond to one of the series, occurring in the above table, we shall be able to take into calculation the personal error found in this particular case.

IV. *Results obtained.*—The rhythm of the sounds of the heart was registered by different persons, almost without exception in the sitting position, sometimes in the state of rest, sometimes after previous bodily exercise. I made use of König's stethoscope, which I found particularly well adapted for hearing the sounds of the heart[a].

It was applied in the region of the stomach, while two elastic tubes proceeding from it reached to the external auditory passages and had their ivory extremities fitted in the same. The fingers of the right hand were joined to one another, just above the marker, while the wrist rested at a suitable height. The rhythm of the cardiac sounds was now imitated by movements of the hand, before the cylinder came into motion, and so soon as it appeared that the rhythm continued regular, and that I had taken it up correctly, the signal was given by a slight movement of the head, whereupon the cylinder was set free, to stop after one circuit. The first observations, in which the cylinder had not acquired sufficient rapidity, were neglected. The heart's action is extremely rarely quite regular. It is therefore not possible to register all the sounds, and I soon confined myself to the alternate registration of α or A, according to method b or c, which methods in testing the accuracy attainable by them, as appears from the above table, were then employed by preference.

To give an idea of the results obtained, I here append the observation of one circuit:

Series I. Mr. A., aged 21, short and stout.

α	2 P.	α in per Centages of P.
9·8	48·2	40·7
9·6	47·8	40·2
9·2	46·5	39·6
9·1	44·3	41·1
9·1	45·	40·4
7·9	43·6	36·2
7·3	40·	36·5
Forward, 62·0	315·4	

[a] In a subsequent communication, treating of the cardiac sounds with reference to the action of the heart, this stethoscope is described.

	a	$2P$	a in per Centages of P.
Over,	62·0	315·4	
	8·3	41·3	40·2
	7·6	41·2	36·9
	7·8	42·	37·1
	8·	40·8	39·2
	7·7	40·3	38·2
	101·4	521·0	consequently $a=38{\cdot}9\%$ of P.

From the seconds registered at the same time I found that 62 pulsations occurred in the minute, consequently $P=\frac{1}{62}'$. In the above series the numbers diminish downwards, which is to be ascribed partly to increasing frequency of the pulsations, partly to retardation of the movement of the cylinder: this too the seconds simultaneously registered shewed us. In the following table we now combine with the first, the results obtained with the subsequent revolutions of the cylinder in Mr. A., in the state of rest (to the modifications produced by movement we shall afterwards revert):

No.	$P=1$:	Method	a' in percentages of P	a' in seconds	Number of Observations	Remarks
1	62	*b*	38·9	0·376	12	(See preceding page).
2	63	*b*	40·8	0·395	9	
3	61	*b*	38·8	0·382	6	
4	63	*b*	40·2	0·383	6	
5	65	*c*	43·5	0·402	7	
6	66	*c*	38·6	0·351	8	
7	64	*c*	42·3	0·397	8	frequency increased by a swallowing movement.

We now combine the average of this Series, obtained in Mr. A., with the averages of the Series found in other persons with greater frequency of the pulse.

Name and age	Series	$P=1$:	α' in percentages of P	α' in seconds	Number of Observations
A. 22	I.	63·4	40·4	0·382	56
Br. 28	II.	74·4	40·6	0·327	59
Jac. 22	III.	77·	41·6	0·324	98
Kw.	IV.	78·	42·6	0·328	53
„	V.	79·7	42·3	0·318	31
Pan.	VI.	81·	44·4	0·329	75
„	VII.	81·5	42·6	0·313	75
St. 2	VIII.	87·7	42·8	0·293	28
L. 22	IX.	89·7	45·1	0·302	29
K. 23	X.	91·	45·6	0·301	61
H. 15	XI.	93·7	41·2	0·309	32

These series lead to the important result that, in the state of rest, α is relatively greater the shorter the period. Hence it appears, that in different persons there is less difference in the duration of the active part of the period, than in the period in its integrity. It seemed to us sufficiently important to calculate the absolute duration of the active part of the period, and we see from the above table that, with the considerable difference in frequency of the pulsations from 74·4 to 93·7, the absolute value of α' continues between 0·327 and 0·301. Only in the first series, where 63·4 pulsations occurred in the minute, does α increase more considerably and indeed to 0·382 second.

We had occasion to determine P and α' in a case of very slow pulse, occurring in John Vroomans, shoemaker, aged 57. Six months ago he had a fit, which he described as a sudden shock in the brain; he had fallen and had got a slight wound in the head, which however had been attended with no further serious consequence. The same day he got two slighter attacks, and subsequently also he continued not quite free from similar seizures. At the same time he suffered habitually from pain in the occiput, was asthmatic, and experienced difficulty in going up stairs. On the first examination he had, when sitting, 32 pulsations in the minute. In the subsequent investigations in which the seconds were at the same time registered, I found at first a still lower figure. We have put the results together in the subjoined Table.

No.	$P=1$:	Method	α' in percentages of P	α' in seconds	Number of Observations
1	29	*b*	15·7	0·325	6
2	38	*b*	20·4	0·322	9
3	37	*b*	19·4	0·315	10
4	35	*b*	17·4	0·307	10
5	35	*b*	18·3	0·314	10

It is remarkable that α', that is the active part of the period, does not in this case last longer than in a pulse nearly three times as quick. A stronger proof of the independence of the duration of the contraction from that of the period could not be adduced. The registration of the sounds of the heart was attended with no difficulty. In the sitting position at rest the heart's action was very regular, and spontaneously the observer was inclined, between each pair of sounds to imagine a second pair, which was not heard: the imitation might take place from the sounds heard just before, as in the ordinary method *b*, with this difference only, that during the imitation the second pair of sounds was absent, whose existence is properly speaking rather prejudicial than advantageous to the observation. The regularity of the heart's action was in this case so great, that, as shall be communicated in a subsequent paper, I even succeeded in making some strokes consecutively coincide precisely with the cardiac sounds.

It is well known that in a standing position the pulse is quicker. In the annexed we find the results in the same person in a sitting and in a standing position compared.

Name and age	Position	$P=1'$:	α'	α' in seconds	Number of Observations	Remarks
Br. 28	sitting	63·4	40·6	0·327	59	
	standing	83·6	41·5	0·298	85	

Evidently with the increased frequency of the periods, in consequence of a standing position, the relative value of α only slightly increases, the absolute on the contrary strongly diminishes. Such observations must, however, be performed on a greater number of persons, in order to be able thence to deduce a general rule.

The results obtained with the altered frequency in consequence of bodily exercise are very remarkable. The individuals were examined in a sitting position after having gone a few times quickly up and down stairs.

In Mr. A., to whom the first series above communicated refers, we found the following:

No.	State	$P=1$:	Method	a'		Number of Observations	Remarks
				in percentages of P	in seconds		
Average	In rest	63·4		40·4	0·382	56	(See above)
1	After exercise	74	*b*	36·5	0·296	9	Some minutes' rest had preceded.
2	1 minute later	68	*b*	33·3	0·294	8	
1′	After repeated exercise	124	*b*	41·2	0·199	15	Immediately.
2′	½ minute later	80	*b*	39·2	0·294	9	Rapid breathing.
3′	1 minute later	70	*b*	34·8	0·298	7	Rapid breathing.
4′	½ minute later	68	*b*	38·1	0·336	7	
5′	2 minutes later	72	*b*	40·1	0·334	7	Breathing much less rapid.
6′	1 minute later	71	*b*	41·4	0·35	9	
7′	½ minute later	67	*c*	39·8	0·356	8	
8′	½ minute later	68	*c*	40·9	0·361	6	

It is remarkable that, while with greatly increased frequency, immediately after exercise (compare 1′), a has become relatively greater, a few minutes later the frequency diminishes considerably (1 and 2, as well as 2′ and 3′), and that at the same time $\acute{a}$ becomes relatively much less. Subsequently the pulse becomes again somewhat quicker and still the relative value of $\acute{a}$ approaches more and more to the state of rest. It occurred to me that the diminished frequency of the pulse, shortly after exercise with quick breathing, might indeed be connected solely with the need of quick breathing. The result of a remarkable experiment which I am unable to refer to its author, but which I first saw with Pflüger, and yearly repeat in my lectures, is pretty generally known. It is this: that in a rabbit whose chest is opened, so long as artificial respiration is kept up, the pulsations of the heart proceed regularly, but that, when the artificial respiration is no longer continued, with the efforts at respiration then setting in, before asphyxia is yet imminent, the heart nearly stops, or rather exhibits very long periods of rest between its contractions. Now the retardation of the pulse, shortly after great exertion, likewise suggests the idea of imperfect respiration and irritation of the nervus vagus. It is, however, possible that the mechanism of respiration itself, with the

accompanying alteration of pressure in the thorax, gives rise to the peculiar retardation. However this may be, the fact is established. The latter acquires still higher signification, when we see how little the absolute duration of $\acute{a}$ is affected by it: this still continues short, when the pulse is only slightly quickened (No. 2 and No. 3′,) and increases from 0·294 to 0·361, without any perceptible retardation of the pulse. The independence of a is thus again exhibited.

I have proved much the same in other persons. The retardation of the pulse becomes shortly after exercise, sometimes so considerable, that with the subsequent increase, $\acute{a}$, although it has become absolutely greater, remains relatively less. We see this in the following example (compare 1, 2, 3):

No.	State	$P=1$:	a	a in seconds	Number of Observations	Remarks
P. average.	Rest	81	44·4	0·329	31	General Table See page 234
1	After exercise	78	36·7	0·282	7	Not immediately.
2	$1\frac{1}{2}$ minutes later	107	40·9	0·229	13	
3	2 minutes later	94	44·7	0·285	6	
1′	After repeated exercise	126	39·7	0·189	15	Immediately.
2′	$1\frac{1}{2}$ minutes later	105	39·4	0·225	15	
3′	$1\frac{1}{2}$ minutes later	98	40.9	0·250	8	

On repeating the exercise 1′, 2′, 3′) the retardation of the pulse did not again occur, but we see the value of $\acute{a}$ relatively and especially absolutely diminish and gradually approach to that of the state of rest.

In the two following young persons too the retardation of the pulse scarcely occurred after exercise, but the relative, and, much more still, the absolute diminution of the value of $\acute{a}$, which regularly approaches that of the state in rest, is in each instance evident.

No.	State	$P=1$:	Method	α' in percentages of P	α' in seconds	Number of Observations	Remarks
K average	In rest	91		45·6	0·301	61	(above x.)
1	After exercise	137	*b*	44·1	0·193	8	
2	0·5 minute later	114	*b*	42·7	0·224	12	
3	After exercise	103	*c*	48·2	0·28	13	
U. average	In rest	937		48·2	0·309	32	(above xi.)
1	After exercise	131	*b*	42·5	0·195	9	29 Oct.
2	0·5 minute later	107	*c*	45·1	0·252	10	
3	1 minute later	110	*b*	48·1	0·262	15	
1′	After exercise	127	*b*	43·8	0·207	9	2 Dec.
2′	0·5 – 1 min. later	126	*b*	44·1	0·21	9	
3′	0·5 min. later	122	*b*	48·5	0·239	8	
4′	1 min. later	108	*b*	46·5	0·258	12	

On the other hand, the retardation of pulse in an asthmatic boy, which, because he was sickly, was not included in the General Table (p. 234) of the averages, but of which, nevertheless, I here communicate, for the sake of comparison, the average in the state of rest, was remarkable. All the observations were made according to method *b*, which, in the end, affords the best results.

No.	State	$P=1$:	α' in percentages of P.	α' in seconds	Number of Observations
H. average	In rest	79·3	47·1	0·356	39
1	2 minutes after exercise	81	37·8	0·287	7
2	0·5 minute later	63	38·6	0·367	

Immediately after the exercise, before registration was possible, the pulse was quick, and at the same time α was nearly equal to A. It was only subsequently that the periods became much greater, with panting

and irregular respiration, and α' was found relatively much less, as is to be seen in 1 and 2. It would be desirable in such cases at the same time to register the respiration, which I shall hereafter not neglect, now that Marey[a] has placed at our command so simple a method of doing so.

Much remains still to be investigated, with respect to the modifications which the rhythm of the sounds of the heart experiences under different circumstances. In particular I have laid out to study in man the influence of digitalis and in animals that of irritation of the vagus. I also hope to present some contributions in the field of pathology. By continued practice I have gradually attained greater accuracy in registering; perhaps also the study of music, which I began early, and have never wholly given up, is not without influence thereupon. The profession has therefore some right to expect that I should proceed with the application of the method to particular cases. Meanwhile I hope in a subsequent communication to show the connexion between the sounds and action of the heart, which has become feasible by simultaneous registration of the impulse and sounds of that organ.

V. *Corollaries.*

1. The cardiac sounds are distinguished as *I* and *II*. The distance from the commencement of *I* to that of *II* is α, that from *II* to *I* is A. The period $P = \alpha + A$.

2. The frequency of the pulse being given, the rhythm of the sounds of the heart teaches us α and $\alpha : P$, that is the absolute and the relative duration of the active part of the cardiac period.

3. The rhythm of the sounds of the heart can be imitated by movement of the hand and so be registered.

4. The accuracy of this method, tested by the imitation of a self-registering rhythm, has been shown to be satisfactory. The probable error is very slight; the personal error is variable with the values of P and $\alpha : P$.

5. In the state of rest α amounts usually to from 0·309 to 0·327 of a second, and continues tolerably equal, with different values of P.

6. In this we have a proof, that α, the duration of the active working of the heart, has a certain independence.

7. $\alpha : P$ is, in the state of rest, the greater, the shorter the periods are; in young persons $\alpha : P = 0{\cdot}404$ to $0{\cdot}482$, average $0{\cdot}428$.

8. With increased frequency of the pulsations in consequence of work (rapid ascent of stairs), α on the contrary often diminishes more quickly than P, and $\alpha : P$ therefore becomes less, the less P is.

9. A few minutes after work is performed, the cardiac periods usually become, with strong and quick respiration, temporarily very long, but α

[a] Journal de l'anatomie et de la physiologie de l'homme et des animaux. Juillet, 1865.

continues in general short, so that then $\alpha : P$ is less than ever. The independence of α is thus again demonstrated.

10. In the standing posture α becomes perhaps relatively somewhat greater, but absolutely considerably less than in the sitting.

On Albuminuria. By Dr. Ernst Œdmansson. From the *Hygiea*, Monthly Journal of Medicine and Pharmacy. Stockholm: May, 1866. Translated by H. Minchin, A.B., M.B., Fellow and Professor of Botany, Royal College of Surgeons, Ireland.

Among the means which may be employed for the detection of albumen in the urine, nitric acid and heat have properly been most constantly and generally employed. The examination of urine by such means was considered at first to be comparatively easy. However, a more close study soon brought to light several sources of error. We have gradually obtained a much clearer insight into the practically-important subject through the valuable labours of Bence Jones and others; but still some questions remain unexplained.

The difference which albumen exhibits in its relation to reagents is as yet uninterpreted. It has generally been considered to depend partly on the existence of varieties of albumen, partly on the operation of other matters which may be present in the urine.

The fact that modifications of ordinary albumen may appear in the urine seems to be brought to light by the well-known observations of Bence Jones, in a case of osteomalacia, where he found an albuminate, which, according to its reactions, could hardly have been the ordinary albumen of serum. This observation, which is now as old as the year 1848, is, however, singular of its kind. Of importance to the decision of the question is the assertion of Gigon, Bechamp, and Harley, that in all fresh urine is found an albuminoïd substance (the "nephrozymas" of Bechamp), which Harley places near to pepton, but which Bechamp proves to be as different from this as from albumen. The supposition of chemically-different forms of albumen may receive some support from a consideration of the fact that albuminuria is capable of being produced by widely different pathological processes within and without the kidney, because by that means slight differences in the composition of the albumen admit of being taken into account as well as those more considerable which are admitted to occur normally within the organism. Experience indeed proves that, in its most palpable features, albumen may react in the same way in various forms of disease; yet a more searching inquiry may lead to the discovery of some constant differences, however slight.

On the other hand, there exists in the urine a solvent of albumen, which undergoes very considerable alterations in composition under

different physiological and pathological conditions. Experiment proves that, by the addition of a material which is already to be found in normal urine, we can alter considerably the reactions of albumen. These may be dissimilar, not only in the same forms of disease, and in cases where the patients have been subject to the same treatment, but even in the same case at different times. The concentration of the urine also exerts a constant influence on certain of these reactions, of which more anon. Investigations pursued in this direction will, in all probability, lead to the conclusion that deviations from the ordinary state, whenever they occur, admit generally of being explained by the composition of the urine.

This variableness in the appearance which the albumen assumes is not merely a matter of theoretical interest, but a knowledge of it is of very great importance to an accurate clinical investigation of the urine. The detection of albumen is, perhaps, not attended with any difficulty when this substance is present in any large quantity; but when its amount is inconsiderable the matter does not always appear so easy. In judging of the quantity of albumen errors are easily committed, unless we proceed according to a method the employment of which will, under all conditions, lead to the precipitation of the entire of the albumen that is present.

It was at first my intention merely to put forward some remarks upon the influence which different degrees of concentration of the urine in certain cases may exercise on the reactions of albumen, and which hitherto appear to have escaped observation. In consideration, however, of the importance of these examinations of urine, and the incompleteness with which they are discussed in the generality of manuals, with the exception of Beale's (*Urine; Urinary Deposits, &c.* London, 1864), my entering somewhat more fully into the subject may possibly be of some service to those who have but lately entered upon the field of these inquiries.

TEST FOR ALBUMEN BY MEANS OF DILUTE NITRIC ACID (ACID. NITR. DILUT. OF THE SWEDISH PHARM. SP. GR. 1·25) AT THE ORDINARY TEMPERATURE.

In the application of this test we proceed either to add five or six drops of this acid to a like number of ccm. of urine in a test tube; or, according to the method of Heller, we allow the acid, in somewhat larger measure (twenty or thirty drops), to flow down along by the side of a glass previously filled to one-third or one-half with urine. In the former case, if the urine contains much albumen a deposit takes place at once on the mixture of the acid with the urine, or else immediately over the acid when this sinks to the bottom; in the latter case the deposition of albumen is usually formed just above the acid, in the form of a smooth disk whose

upper and lower surfaces are defined by a more or less sharp outline. A modification of Heller's mode of proceeding consists in the cautious addition of the urine to the acid previously iced in the glass. Of the two principal methods, that of Heller deserves a decided preference, because (1) the reaction elicited by this method is, in general, far more characteristic; and (2), according to the others, if the amount of albumen in the urine be inconsiderable, we often obtain only a diffuse cloudiness, while in some cases we perceive no reaction if we use merely a couple of drops of acid; whereas the test in the same case, applied according to Heller's method, will give a manifest and definite result; and (3) the other is not applicable to cases in which the urine contains a large quantity of lithates.

This test is applied in such a way that the acid is allowed to run down along the side of a test-tube containing urine, and it is only a modification, or rather a corruption, of Heller's procedure. The only advantage it has is that we can immediately, at intervals, employ the same glass and the same urine for the further test by means of heating. In such cases, however, we should add somewhat more of acid than the quantity above directed—say about 10-15 drops to 5-6 ccm. of urine.

A good test for albumen, where this is present in large quantity, consists in adding the nitric acid guttatim to the urine previously introduced into a tall glass cylinder. After the first few drops of acid, a heavy precipitate is formed, which sinks down with tolerable rapidity, and is usually redissolved afterwards. Gradually, as more acid is added, the solubility of the precipitate is diminished, and it finally settles down at the bottom of the glass in the form of small rounded or irregular masses, which have some resemblance to the chloride of silver precipitated under similar circumstances. The precipitate sometimes sinks at once heavily to the bottom. These characters of the deposit—namely, its form, heaviness, and its solubility while sinking, constitute altogether a peculiarity which does not belong to any of the usual normal constituents of urine.

The albuminous disk obtained by Heller's method, or its modification, takes its place, as we have said, immediately over the acid; but, where the albumen is only to be found in small amount, and the sp. gr. of the urine is not very low, it may be observed not unfrequently to lie at a higher level in the vessel. Its density stands, moreover, in a direct relation to the quantity of albumen present, while its volume is not defined in this way alone. It may be so pellucid, in specimens containing a very small proportion of albumen, that it can only be rendered visible in certain lights; and in some cases it becomes evident only after the lapse of one or two minutes, for which reason we ought always to wait some little time before the experiment can be said to have given a negative result. Sometimes we find above the acid only a slight

cloudiness without any defined upper limit, and this might easily lead us to suppose that the specimen is free from albumen. This, however, does not happen unless we perform the experiment with a specimen which was muddy at the commencement; we should, therefore, adopt the mode of filtering the urine in every case where it is not clear.

With this test a layer of precipated lithates is usually formed nearer to the free surface of the urine, and this layer is separated from the albumen by a more or less clear interval—a circumstance which in a high degree betokens the characteristic nature of the test. The upper surface of this layer is also uneven, but may, sometimes, when the layer is thin, be not more uneven than the surface of the albumen. Sometimes both layers become mixed or melted together, and thus the test becomes frustrated.

We may be quite certain of finding this reaction if the urine contains albumen, provided that the precaution above stated has been attended to. On the other hand its appearance merely declares that in all probability there is albumen present, for this substance may be simulated by other things; lithates, for example, especially if we use but a small quantity of urine, and if we do not add a sufficiency of acid. In general, however, the lithates ought not to mislead, while the test must always be regarded as of little value, unless the reaction be very evident; that is to say, unless we find a clearly defined deposit to lie immediately above the acid, or, as is usually the case, we find two layers divided by a clear interval. The agglutinated crystalline mass of nitrate of urea, which is sometimes suspended immediately above the acid, is, with a little attention, easily distinguished from the albumen along with which it sometimes appears. On the contrary, lithic acid may occasionally fall down, and form just above the nitric acid a layer which might easily be mistaken for albumen. Along with this, and higher up in the liquid, may float a layer of lithates, whereby the risk of error is augmented in a high degree. Sometimes three layers are formed—a lower one of albumen, and two, lying close to one another, above, which latter soon become amalgamated into one. If we remove the albumen, by heating and filtration, from another portion of the same urine, and afterwards repeat the experiment with the filtrate, we shall obtain two well-defined layers, of which the lower consists of lithic acid. The lithic acid layer has for the most part a faintly brownish tint. This lithic acid is not always amorphous, as some authors maintain, for, upon examining it immediately with the microscope, we can detect numerous well-developed crystals, which, however, are very minute, and of variable form. The substance, to the formation of which the use of copaiba, cubebs, and matters akin to these, sometimes gives rise, may tend to mislead with this test; an example of which will be adduced further on.

TEST FOR ALBUMEN BY HEATING, WITH OR WITHOUT THE ADDITION, OF NITRIC ACID.

In a specimen of urine which contains albumen, the application of heat usually gives rise to a precipitate of albumen if the urine be acid; none, or an imperfect one, if it be alkaline. We ought, therefore, before performing this test, to ascertain whether the urine is acid, and if it be not so, to add a small quantity (one or two drops) of dilute acetic acid, in order to communicate a slight degree of acidity.

The point of coagulation of albumen is subject to very considerable variation. If this substance be present in large quantity, a deposit makes its appearance after a slight application of heat, and first in that part of the urine which is nearest to the source of heat; in other cases a higher temperature must be employed, whereupon the deposition begins to form at the free surface of the liquor. When the proportion of albumen is very small, there appears frequently, after a rapid ebullition of the urine, only a mere opalescence or cloudiness, from which, after the urine has cooled, there sometimes subsides a distinct precipitate; occasionally, however, it fails even then to appear. Less frequently we meet with urine, which, notwithstanding that it contains albumen, and has an acid reaction, continues still clear even after prolonged boiling. In the cases in which I have observed this condition, the urine had a pretty high specific gravity (1·019, or higher), and generally a strongly acid reaction, with a high colour; the albuminuria has existed along with some acute disease, such as pneumonia, typhoid fever, &c. According to Bence Jones, who first observed this phenomenon, authors have pretty generally adopted the opinion that this depends upon the presence of free nitric acid in the urine. We find, however, similar cases, in which the patients have not been taking any nitric acid or nitrates, while, on the other hand, we may administer considerable doses of this acid without communicating to the urine the property in question.

In fact, we meet with all transitions among cases—we are only alluding to those in which the urine contains a minute quantity of albumen—from that where the albumen is precipitated by a gentle application of heat, to one in which no coagulation can be effected. This unequal tendency to precipitation by heat does not stand in any definite relation to the quantity of albumen which is present. It cannot generally depend on a dissimilar chemical composition of the albumen, for it is met with in several cases of albuminuria arising from the same cause, and even in the same case at different times. We may, therefore, assume that it probably has its origin in the variable composition of the urine. The concentration of this fluid appears to exercise a certain influence, for the albumen, in general, has a greater tendency to precipitate in specimens of a low than

in those of a high specific gravity. The reason of this, in all probability, is that the material, whatever it may be, that prevents the precipitation is, as a rule, present in much larger proportion in a concentrated urine. The degree of acidity in the urine is certainly an important item; however, a urine which contains an easily coagulable albumen may be equally acid with one of difficult coagulability. The principal question perhaps is, upon what acid or acids does the acidity of the urine depend? On some occasions I believe I have been able to remark that the coagulability of the albumen has been diminished during the acid fermentation of the urine.

The experiments I have contrived, with a view of altering the relation of the albumen to the action of heat, by adding to the urine a solution of the saline matters which enter normally into its composition, have not as yet led to any certain result. Acid phosphate of soda prevents, indeed, the precipitation of albumen; but with respect to the quantity of the salt which is required for the purpose it is worthy of remark, that when a few drops of a stronger solution are sufficient for a urine of a low specific gravity, a quantity four or five times larger will be required when the urine is more strongly concentrated, even though its proportion of albumen be tolerably small. Even the degree of acidity which the urine possesses will appear to exert some influence, requiring the addition of much of the saline solution. Thus I have found sometimes that of two specimens of urine, having a low specific gravity, and a considerable amount of albumen, but of which one was strongly acid, and the other neutral or weakly acid, the former remained clear upon boiling, after the addition of a much smaller quantity of salt solution, than the latter; this tends strongly to establish the inference that the former contained a larger amount of phosphates than the latter. From this it will appear that in such cases the phosphoric acid already present in the urine, adds its efficacy to that which is subsequently added. That it is less in a condition to hold the albumen in solution, when the urine is more strongly concentrated, is shown by the counteraction of its efficacy by means of another material. The discovery of this ought to supply the key to an accurate estimation of the cause of the dissimilar bearing which the albumen of urine exercises towards heat.

In order the more completely to precipitate the albumen, we are often in the habit of adding a little acetic acid before applying heat. This mode of proceeding, however excellent where the quantity of albumen in the specimen is inconsiderable, is frequently insufficient when an opposite condition is present. It will answer just as well to use a little nitric acid; but here, even more than in the case of acetic acid, we should take care to employ an excess, in order to avoid the production of the very contrary effect to that intended; that is to prevent the precipitation. A better plan than either of these is—before the application of

heat, to make the urine strongly acid by means of acetic acid, and then to add to it a concentrated solution of sulphate of soda, a procedure which has been highly recommended by Hoppe. Chloride of sodium will answer the purpose equally well.

As the albumen does not always fall down upon the application of heat to an acid urine, since the precipitate that takes place frequently fails to correspond to the amount of albumen present in the urine; and whenever also, the phosphates can be thrown down by the boiling of even considerably acid urine, it will be necessary, frequently, that we make further trials, in order to be certain whether it be present, and in what quantity. This consists in the addition of nitric acid to the boiled liquid. Now, it is generally stated in the books that if the precipitate thereby produced does not re-dissolve, it consists of albumen; in other cases, of phosphates. In this, however, there is a great error, inasmuch in the cases in which the sp. gr. of the urine is low, the precipitated albumen can be completely re-dissolved by the addition of one or two drops of nitric acid to the boiling urine. The solubility of the precipitate stands in an inverted relation to the sp. gr. of the urine, and ceases, as far as my experience extends, when the urine has a density of 1·016, or thereabouts, even for minute quantities of albumen. At a higher concentration a small precipitate is partially re-dissolved on some occasions, or may disappear upon heating, but only to reappear as the liquid cools. We may sometimes meet with specimens of urine of a somewhat lower sp. gr., 1·013—1·014, in which even a little precipitate does not dissolve. The solubility appears to be greater, *cæteris paribus*, where the urohematin is present in small quantity, as is usually the case, in chronic renal affections, than where the colouring matter is more abundant. It may be greater in an originally highly acid than in a neutral, or faintly acid urine, even though the former should have the same proportion of albumen; nay, even if it should have a somewhat higher specific gravity. When we find in this way that a large quantity of phosphatic salt is present in the urine, it would appear that the phosphoric acid can, in some mode or other, contribute to the solubility of the precipitated albumen, as it has shown already to be probable that, under the same circumstances, that is, when the urine has a low specific gravity, it may counteract its precipitation.

In order that the precipitated albumen may be dissolved by the addition of nitric acid, it is necessary that the urine be still boiling hot or that it be boiled again. Even if the precipitate be completely cooled down, it re-dissolves upon heating it with a little acid. This will take place whether the acid be added before or during the boiling. In performing experiments of this kind it is of the highest importance that we employ a due quantity of acid, but this may vary in some small degree under dissimilar conditions. If we continue to add the acid by drops to the urine previously cleared, this fluid begins soon to grow muddy again,

and the precipitate increases until a certain quantity, usually 12 or 15 drops, has been added.

By diminishing the concentration by adding distilled water, we can in each case of albuminous urine, arrive at a point at which the precipitate obtained directly by heat becomes soluble on the addition of a small quantity of nitric acid, whereas previously this was not the case. From these experiments, then, it would appear that the solubility of precipitated albumen is very closely connected with the concentration of the urine.

Some authors have advised the employment of hydrochloric or acetic acid in place of nitric, in testing for albumen, thereby assuming that albumen is absent if the precipitate produced by heat be soluble in one or two drops of either of these acids. As far as respects the hydrochloric, this supposition is not correct, because this acid is able, under the same conditions as nitric, to dissolve the precipitate; but nitric acid is less to be depended upon, as this does not, in such minute quantity, exercise a solvent action upon albumen.

The deposit which occurs in albuminous urine when nitric acid is added at the ordinary temperature, re-dissolves, for the most part, completely upon stirring, if we use only one or two drops of acid. Urine treated in this way usually remains clear upon being heated. Beale assumes this last condition to be constant, but I have sometimes met cases where, notwithstanding the observance of the strictest precaution in the addition of the acid, the urine still grew cloudy on the application of heat, whilst in other cases it was quite clear at a high temperature, but became cloudy as it cooled. In these cases the urine had always a high specific gravity. After mixing with the urine a certain quantity of a concentrated neutral saline solution, nitric acid ceased to hold the albumen in solution upon heating the urine. The greater the proportion of albumen in the urine in such case, the less of saline solution is required to be added. The quantity of acid which is generally necessary in order to keep the urine clear upon heating varies in some degree, for which reason much caution must be used in experimenting according to this method. A trace of nitric acid or a slight excess causes little or no hindrance to the precipitation. Sometimes the urine clears upon heating, even though a slight cloudiness was found at ordinary temperature. Without requiring to know this dissimilar operation of nitric acid, we ought, therefore, never, in ordinary experiments on urine, to add the nitric acid before boiling, and always to be very careful how we employ a test tube, in which some acid from a previous experiment may be remaining.

This solubility of albumen by heat, after the addition of a little nitric acid, is explained by Bence Jones, by saying that the albumen forms a compound with the acid, which, even at high temperatures, is soluble in

very dilute acid; whilst Beale endeavours to show, by means of experiment, that nitric acid expels phosphoric acid from its combinations, and that the latter, as long as no excess of nitric acid be present, retains the albumen in solution.

It has been shown by what precedes, that we cannot infer the absence of albumen, if the deposit produced by boiling becomes re-dissolved by heating with only a few drops of nitric or hydrochloric acid. A somewhat larger amount of nitric acid must be employed. In general it may be assumed that 12–15 drops will throw down all the albumen from the quantity of urine usually operated on in these experiments, even though the original precipitate appeared to dissolve on the addition of the first few drops. In cases where the proportion of albumen is small, it is often necessary, after adding the acid, to boil the sample again, otherwise it may happen that the urine will only exhibit more or less of cloudiness, and that, even if the specimen be allowed to stand for some time, no actual precipitate will form. Even after the last boiling we must often, where it is a question of a small quantity of albumen, defer judging of the result until the urine has quite cooled, because the deposition at the higher temperature may be incomplete, and in some cases is hardly observable. Having effected, in this way, a complete precipitation of the albumen, we find this not unfrequently to exist in a much greater quantity than we should have been led to expect according to the preceding experiments.

The albuminous deposit, obtained by one or other means, is soluble on heating it with excess of nitric acid. On allowing, then, the deposit to settle down, pouring off the clear liquor and boiling the residue with its own volume, or, better still, with twice its volume of nitric acid, the precipitate is re-dissolved.

From all that has preceded, it follows that an albuminous urine may be affected in the most dissimilar ways by boiling, with or without the addition of nitric acid; and nothing but an accurate acquaintance with these facts can, under all circumstances, guard against errors, especially in cases where the albumen happens to be present in minute quantity. The best mode of proceeding in the examination of urine is the following:— One part of urine, which, if possible, should be filtered, is tested according to the method of Heller, or the modification of it already spoken of, which latter not unfrequently gives a sharper outline to the layer of albumen than the former. If the reaction be evident, although ever so slight, then in all probability the urine contains albumen; in case of its non-appearance, we cannot be sure until after some minutes. We ought, if necessary, to examine the specimen in different lights. Another portion of the urine is boiled in a test tube, after we have ascertained its reaction with litmus paper, and if it be not distinctly acid, we add one or two drops acetic acid. Should no deposition make its appearance or

merely an opalescence, upon the first boiling, the liquid should be boiled for some time longer. Whether a precipitate forms now or not, 12 or 15 drops of nitric acid are added, after which it may, perhaps, be necessary to boil again. In doubtful cases the test is not decided until after the urine has cooled. If the experiment is to be performed rapidly, it is best, omitting other tests, to boil up the urine, add 12 or 15 drops of nitric acid, and boil again. When the albumen is present in large quantity, the test of Heller will be found sufficient, with a single heating of the acid urine. It may occasionally be of advantage to control Heller's by the test previously stated, in which the acid is poured guttatim into a large quantity of urine, as for, example, when we have not a spirit lamp at hand.

Case of Renal Disease, in which, during the use of copaiba, a peculiar substance appeared in the urine.

A soldier was admitted, some time ago, into the Garrison Hospital, labouring under sub-acute nephritis, without dropsy or symptoms, arising from affection of any other organ than the kidneys. The bleeding from the kidneys had ceased; the albuminuria, likewise the quantity of renal epithelium mixed with the urine, had diminished, when one day there appeared in the sediment a large number of pus-corpuscles and epithelial cells of various forms, such as are found in the upper part of the urinary tract. No signs of vesical irritation were present. After other remedies had been employed without effect, Bals. copaib. was administered in doses of twenty drops, three or four times a day. The cell-formation and the albuminuria began soon to be diminished, and, during several days, had been reduced to a very trifling amount, when the patient, after some time, was discharged in order to be further treated at home. On the day that he went out—he had been then taking copaiba for seventeen days—I examined the urine, and found to my astonishment that the test of Heller indicated the presence of a considerable quantity of albumen. A further examination of the urine gave the following result:—First—It was light yellow in colour, somewhat muddy, of a density of 1·022, with an acid reaction. Second—The deposit effected by the test of Heller was homogeneous, of a white colour, and lay immediately above the acid. Third—Acetic acid, nitric and hydrochloric, produced in the urine a considerable cloudiness, from which, in three days afterwards, no precipitate of lithic acid had fallen. The urine became again clear, on the addition of an excess of concentrated acetic or hydrochloric acid. Fourth—By direct and prolonged boiling, a very small precipitate was formed, which did not quite sink to the bottom. Upon warming the urine with the addition of a little acetic acid, it did not become clear; at a higher temperature the muddiness was somewhat increased. The opalescence which arose

from the addition of some few drops of nitric acid, did not disappear either by shaking or heating the test tube. Fifth—The precipitate caused by direct boiling, was not altered by one or two drops of nitric acid, but by boiling with fifteen drops of the acid it became more considerable. It was brown in colour, had the usual appearance of albumen, under the microscope, and became yellow when treated with dilute solution of iodine. This precipitate was probably somewhat more abundant than that which the urine a few days previously afforded in a similar experiment, but did not correspond to the quantity which the test of Heller now showed to be present. The urine became clear after several days.

No further experiments could be made, as, unfortunately, I had only obtained a small quantity of the urine.

In a week afterwards I had an opportunity of examining the urine of this patient, and I found it to contain then a minute quantity of albumen presenting the normal reaction.

BOOKS RECEIVED, FEBRUARY, 1868.

1. Germinal matter and the contact theory. An essay on the morbid poisons, their nature, sources, effects, migrations, and the means of limiting their noxious agency. By James Morris, M.D., Lond., &c. Second edition. London: Churchill, 1867. Post 8vo, pp. 111.

2. On Ringworm. An inquiry into the pathology, causes, and treatment of the several diseases to which this term has been applied. By Wm. J. Smith, M.B., &c. London: Hardwicke, 1867. Post 8vo, pp. 75.

3. Excision of the knee-joint. A description of a new apparatus for the after treatment, with illustrative cases. By Patrick Heron Watson, M.D., &c. Edinburgh: Maclachlan and Stewart, 1867. 8vo, pp. 78.

4. The journal of anatomy and physiology. Second series. No. I. Macmillan & Co. 8vo, pp. 200.

5. The nature and affinities of tubercle; being the Gulstonian lectures for the year 1867. By R. Southey, M.D., &c. London: Longmans, 1847. 8vo, pp. 118.

6. The Galveston Medical Journal. Vol. XI. No. 10.

7. A contribution to the history of the hip-joint operations performed during the late civil war; being the statistics of twenty cases of amputations and thirteen of resections at this articulation, in the Southern service. By Paul F. Eve, M.D., Philadelphia: Collins, 1867. 8vo, pp. 17.

8. On the physiological action of the Calabar bean, physostigma venenosum (Balf.) By Thomas Fraser, M.D. From Trans. of Royal Society. Edinburgh, 1867. 4to, pp. 73.

9. Scleroderma and its relations to elephantiasis arabum. By Dr. V. Rasmussen. Translated by W. D. Moore, M.D., &c. Reprint from Edinburgh medical journal.

10. Report on leprosy, by the Royal College of Physicians. Prepared for her Majesty's Secretary of State for the Colonies. With an appendix. London: Eyre and Spottiswoode, 1867. Fcap. pp. 244.

11. Transactions of the pathological society of London. Vol. XVII., 1867. 8vo, pp. 317.

12. The physiology of man, designed to represent the existing state of physiological science, as applied to the functions of the human body. By Austin Flint, jun., M.D., &c. Alimentation, digestion, absorption, lymph, and chyle. New York: Appleton and Co., 1867. 8vo, pp. 556.

13. On the distinctive characters of external inflammations. On inflammatory or sympathetic fever, and the results of thirty-six years' experience of the effects of bleeding, ascertained from private practice only. By J. H. James, F.R.C.S., &c. London: Churchill, 1867. 8vo, pp. 104.

14. Lectures on the progress of anatomy and surgery during the present century. By Sir W. Fergusson, Bart. London: Churchill, 1867. 8vo, pp. 302.

15. A clinical study of stimulation in typhus, with incidental remarks on its clinical history. By James B. Russell, B.A., M.D., &c. Reprint from Glasgow medical journal.

16. Catalogue of the United States Army Medical Museum. Prepared under the direction of the surgeon-general U.S. Army. Surgical section by Alfred A. Woodhall, assistant-surgeon and brevet-major U.S. Army. Medical section by brevet lieut.-colonel J. J. Woodward, assistant-surgeon U.S. army, in charge of the medical and microscopical sections of the museum. Microscopical section by brevet-major Edward Curtis, assistant-surgeon U.S. army. Washington Government Printing Office, 1866–7. 4to, pp. 664, 136, 160.

17. Handbook of physiology. By W. Senhouse Kirkes, M.D. Sixth edition. Edited by W. Morrant Baker, F.R.C.S., demonstrator of anatomy and operative surgery, &c., St. Bartholomew's Hospital, London. Walton, 1867. Post 8vo, pp. 802.

18. The practice of medicine and surgery applied to the diseases and accidents incident to women. By W. H. Byford, A.M., M.D., &c. Second edition. Philadelphia: Lindsay and Blakiston, 1867. 8vo, pp. 605.

19. The Liverpool medical and surgical reports. Oct., 1867. London: Churchill, 1867. 8vo, pp. 158.

20. On State medicine in Great Britain

and Ireland. By Henry W. Rumsey, M.D., &c. London: Ridgway, 1867. 8vo, pp. 58.

21. The British Journal of homeopathy. No. CII.

22. The British Pharmacopœia, 1867, considered: being the inaugural address for the thirteenth session of the school of medicine of the Catholic University of Ireland. By S. M. MacSwiney, M.D., &c. Dublin: Fowler, 1867. 8vo, pp. 14.

23. Journal of the Scottish meteorological society. Oct., 1867. No. CII.

24. The North British Review. No. XCII. Dec., 1867.

25. Report of the proceedings of the association of medical superintendents of American institutions for the insane, 1867. Harrisburg: Scheffer. 8vo, pp. 132.

26. Proceedings of the Essex institute. Nos. 3 and 4.

27. Annual report of the council, and proceedings of the Halifax literary and philosophical society, 1867.

28. Cottage hospitals, their objects, advantages, and management. By Ed. J. Waring, M.D., &c. London: Churchill, 1867. 8vo, pp. 40.

29. Nyare undersökningar och iakttagelser rörande de fysiologiska och terapeutiska verkningarne af bad i förtätad luft. Meddelade af Dr. Oskar Th. Sandahl (Aftryck ur *Hygiea*). Stockholm, 1865.

30. Berättelse, afgifven till Kgl. Sundhets-Kollegium om den Medikopneumatiska Anstaltens verksamhet i Stockholm. Under åren 1863, och 1864. Af Dr. O. T. Sandahl.

31. Des Bains d'air comprimé. Court aperçu de leurs effets physiologiques et thérapeutiques précédé d'une description de l'établissement médico-pneumatique de Stockholm. Par O. T. Sandahl.

32. The fourteenth annual report of the Rescue Society, otherwise called the society for the rescue of young women and children, ending 31st March, 1867.

33. Handbook of the Sphygmograph; being a guide to its use in clinical research; to which is added a lecture delivered at the Royal College of Physicians, on the mode and duration of the contraction of the heart in health and disease. By J. B. Sanderson, M.D., F.R.S. London: Hardwicke, 1867. Post 8vo, pp. 83.

34. Zur Lazarethfrage. Erwiederung von Prof. von Dumreicher an Prof. von Langenbeck. Wien, 1867. 8vo, pp. 77.

35. Neligan's medicines, their uses and modes of administration. By Rawdon Macnamara, Prof. Mat. Medica Royal College of Surgeons in Ireland, &c. 7th ed. Dublin, Fannin and Co. 1867. 8vo, pp. 934.

36. On the pathology and treatment of albuminuria. By W. H. Dickenson, M.D., &c., Assist. Phys. St. George's Hospl. 8vo, with plates. London: Longmans, 1867.

37. Outlines of physiology, human and comparative. By John Marshall, F.R.S., Prof. of Surgery University College, &c. 2 vols. London: Longmans, 1867. Crown 8vo, pp. 607 and 699.

38. On the diagnosis, pathology, and treatment of diseases of women, including the diagnosis of pregnancy. By Graily Hewett, M.D., Prof. of Midwifery and Diseases of Women, University College, &c. 2nd ed., revised and enlarged, with numerous illustrations. London: Longmans, 1868. 8vo, pp. 700.

39. Journal of cutaneous medicine, No 4. Jan., 1868.

40. Remarks on the climate of Sidmouth, with tables giving the results of meteorological observations. By J. T. Mackenzie, M.B. London: Churchill, 1867. 8vo, pp. 32.

41. Progress in school discipline; corporal punishment in the public schools. Addressed to the citizens of Cambridge. By Morrell Wyman, M.D. Cambridge: J. Wilson and Sons, 1867. 8vo, pp. 48.

42. Examination papers for the degree of M.B., Oxford University.

43. A lecture delivered at the Meath Hospital on the occasion of the opening of the session, Nov. 4, 1867. By M. H. Collis, M.D., F.R.C.S.I., &c. Dublin: Falconer. 8vo, pp. 31.

44. Extern maternities and lying-in hospitals. By L. Atthill, M.D., &c. Reprint from *Medical Press*, 1867.

45. Atlas of portraits of diseases of the skin, issued by the New Sydenham Society. 7th Fasiculus.

46. The diagnosis and treatment of the varieties of dyspepsia considered in relation to the pathological origin of the different forms of indigestion. By Wilson Fox, M.D., &c. 2nd ed. London: Macmillan and Co. 1867. 8vo, pp. 247.

47. The British journal of homeopathy. January, 1868.

48. The surgery of the rectum, being the Lettsomian lectures on surgery delivered before the medical society of London, 1865. By Henry Smith, F.R.C.S., &c. 2nd ed. London: Churchill. 1868. Fcp. 8vo, pp. 152.

49. On ovarian dropsy, its nature,

diagnosis, and treatment. By I. Baker Brown, F.R.C.S., &c. 2nd ed. London: Hardwicke. 1868. 8vo, pp. 280.

51. The First principles of modern chemistry. A manual of inorganic chemistry. By U. Kay-Shuttleworth. London: Churchill. 1868. Crown 8vo, pp. 214.

51. On the co-existence of two species of inflammation, with special reference to the forms of pneumonia. By Wm. Addison, M.D., F.R.S., &c. London: Churchill. 1868. 8vo, pp. 45.

52. The first step in chemistry. A new method of teaching the elements of the science. By Robert Galloway, F.C.S., &c. 4th ed. Re-written and enlarged, with illustrations on wood. London: Churchill. 1868. Fcp. 8vo, pp. 477.

53. On the application of sulphurous acid, gaseous and liquid, to the prevention, limitation, and cure of disease, with cases illustrative of the advantages to be derived from its employment. By James Dewar, M.D. 7th ed. Edinburgh: Edmonston and Douglas. 1868. 8vo, pp. 47.

54. The great sulphur cure brought to the test, and workings of the new curative machine proposed for human lungs and windpipes. By Robert Pairman, surgeon, Biggar. 7th ed. Edinburgh: Edmonston and Douglas. 1867. 8vo, pp. 45.

55. On spinal weaknesses and spinal curvatures, their early recognition and treatment. By W. J. Little, M.D. London: Longmans. 1868. 8vo, pp. 121.

56. Medico-chirurgical transactions. 2nd series; Vol. XXXII. London: Longmans, 1867. 8vo, pp. 649.

57. Die optischen Fehler des Auges mit ihren Folgen Asthenopie und Strabismus, von J. Z. Lawrence, F.R.C.S., aus dem Englischen ins Deutsche übersetzt und mit erweiternden Zusätzen versehen von Dr. August Karst, Kreuznack, Voigtländer, 1868. 8vo, pp. 163.

58. Archives de physiologie normale et pathologique publiées, par M. M. Brown-Séquard, Charcot, Vulpian, No. 1, Janvier-Février, 1868. Paris: Masson et Fils. 8vo, pp. 206.

59. Guy's Hospital Reports, 3rd series. Vol. XIII.

60. Abhandlungen herausgegeben vom naturwissenschaftlichen Vereine zu Bremen. Bremen: Müller, 1867. 8vo, pp. 222.

61. The Quarterly Journal of physiological medicine and jurisprudence. Edited by W. A. Hammond, M.D. No. 1. Vol. II. January, 1868. New York: Moorhead, Simpson, and Bond, 1868. 8vo., pp. 207.

62. The world as dynamical and immaterial, and the nature of perception. By R. S. Wyld, F.R.S.E. Edinburgh: Oliver and Boyd, 1868. Crown 8vo, pp. 227.

63. On certain moral aspects of money-getting. By W. T. Gairdiner, M.D. Glasgow: Maclehose, 1868. 8vo, pp. 47.

CASE OF KELLY.

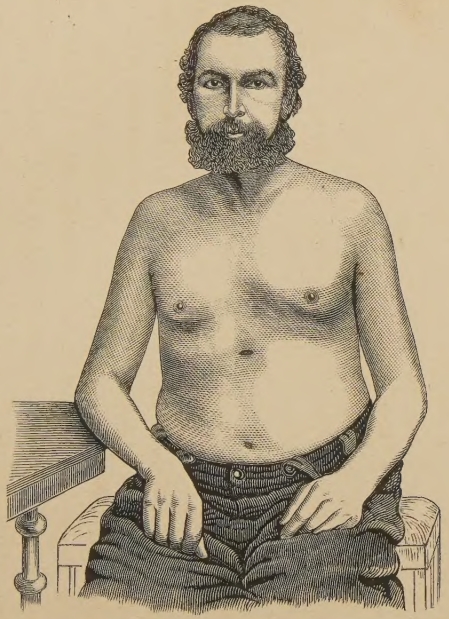


Fig. I.—Showing where the Bullet entered.

Fig. II.—Showing position of Wound made for extraction of Ball.